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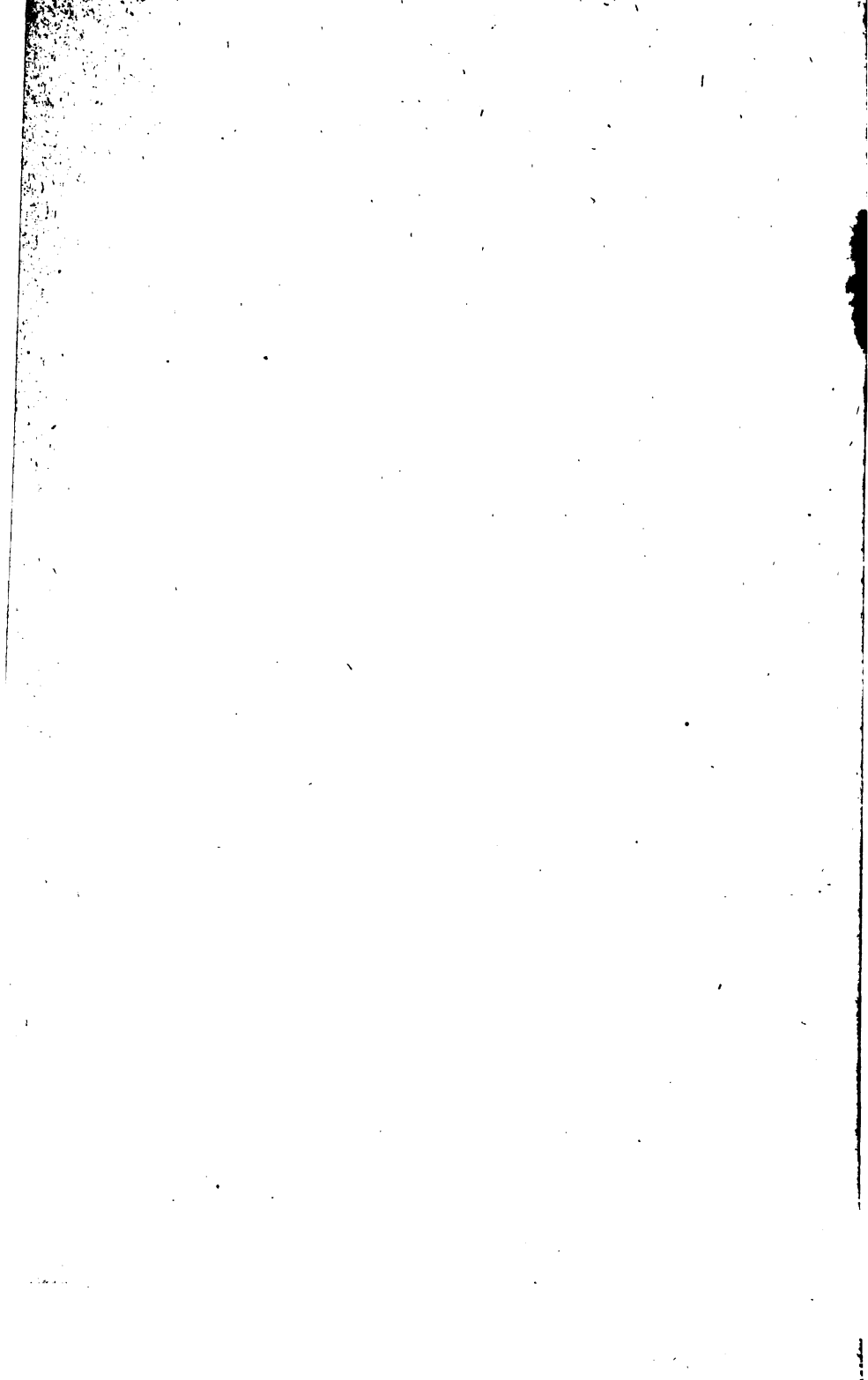
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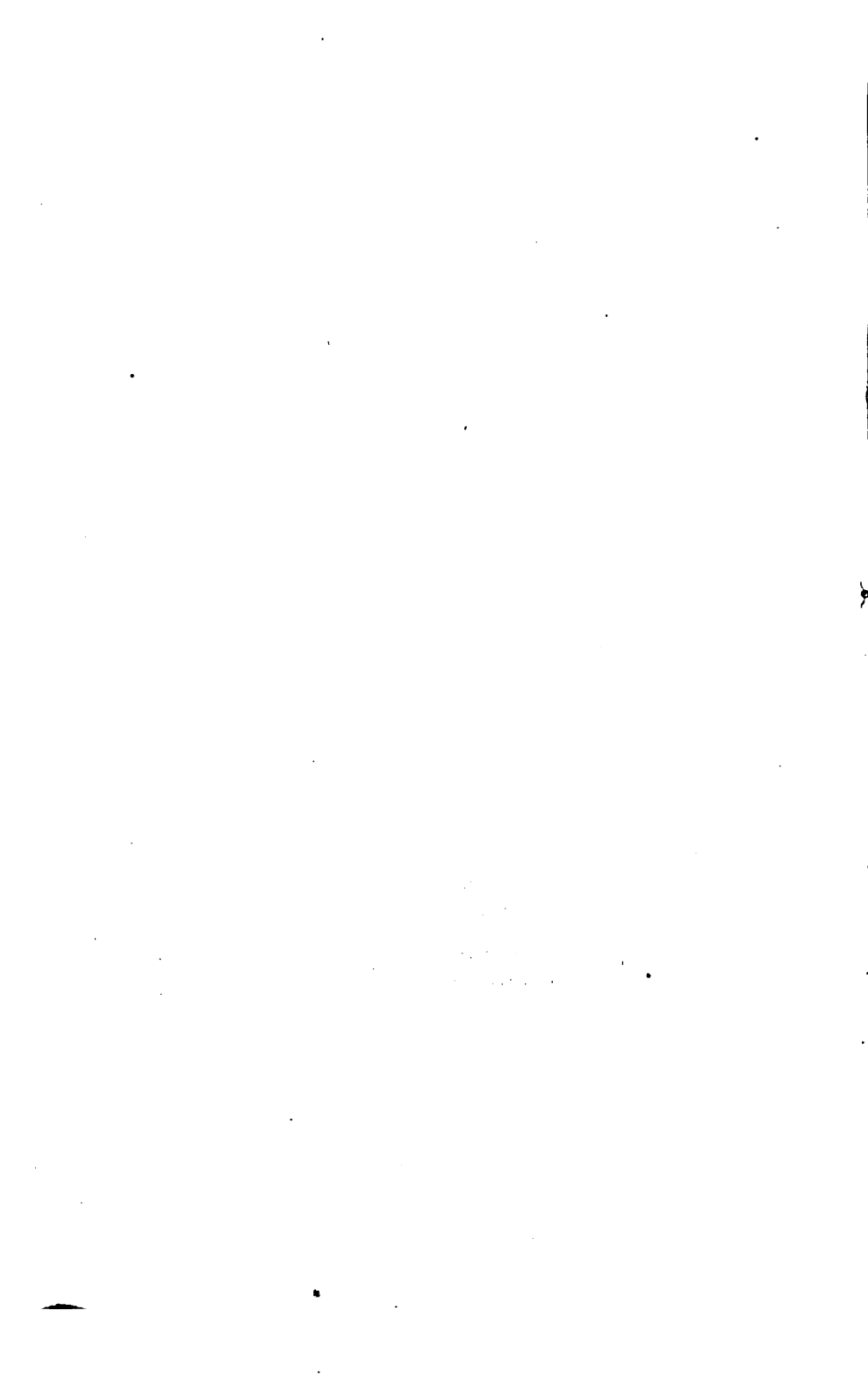
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PREFACE.

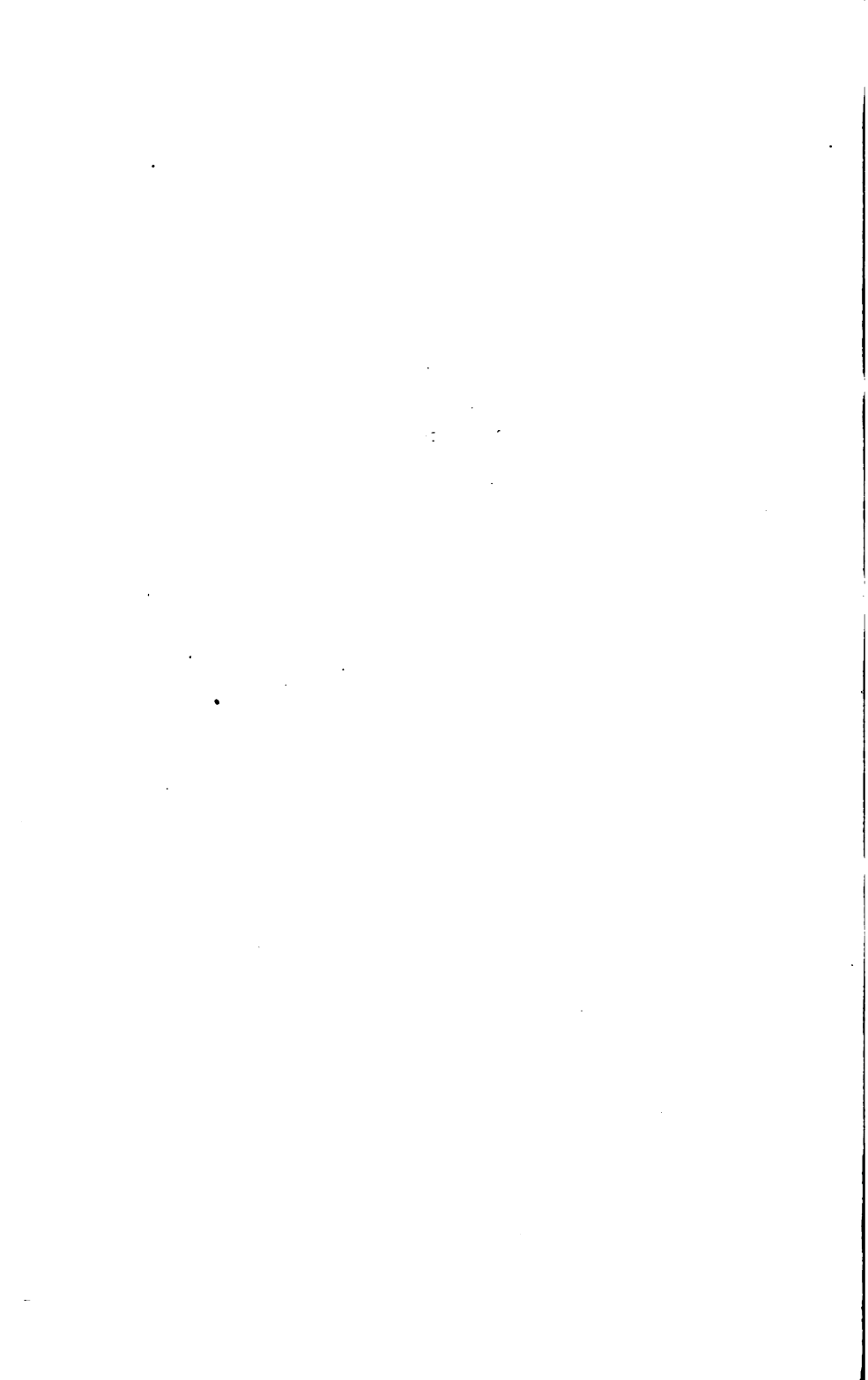
The next volume of the Reports will be published in July, and will contain papers by Drs. BARLOW, WALLACE, and Mr. G. E. WALKER, which but for lack of space should have appeared in this number. Papers have also been promised by Messrs. BICKERSTETH, REG. HARRISON, SHADFORD WALKER, MITCHELL BANKS, EDGAR BROWNE, THOMAS, and RUSHTON PARKER; and by Drs. DRESHFIELD, (to whom I am much indebted for help in the Manchester department,) DAVIDSON, GLYNN, &c., &c.

J. W.

*Gambier Terrace,
Dec. 1877.*

CONTENTS.

	PAGE
On Past and Present Treatment of Intestinal Obstructions, by HUGH OWEN THOMAS	I
Bacteria not Essential to Infection, by JAMES ROSS, M.D., M.R.C.P., Manchester	59
Some Results of Lead-Impregnation, by Geo. SHEARER, M.D., Liverpool	74
Turning.—Notes of Forty Cases with Remarks, by F. H. V. GROSHOLZ, L.K. & Q.C.P., Irel., &c.	92
On Chronic Atrophic Rhinitis, by J. DIXON MANN	97
Gleaning from Obstetric Cases, by J. ARMSTRONG, M.B.	104
Cases from Surgical Practice:—(Two Cases of Ovariectomy with Remarks,) by ANDREW BONTFLOWER	111
Notes on the Pathology of Plastic Bronchitis, by RICHARD CATON, M.D.,	120
Note on an Improvement in the Short Hinged Forceps, by FRANCIS VACHER, Birkenhead	124
Remarks on Venesection with Cases Illustrating its Value, by T. R. GLYNN, M.B. London, M.R.C.P.	126
On the Phenomenon of So-called "Direct" Paralysis, by EDWARD H. DICKINSON, M.A., Oxon., M.R.C.P.	141
A Plea for the More Free Removal of Cancerous Growths, by W. MITCHELL BANKS, F.R.C.S.	192
Four Cases of Antiseptic Osteotomy, by W. M. CAMPBELL, M.D., M.R.C.S.	207
Abstract of the Proceedings of the Liverpool Medical Society, by.. CHAUNCEY PUZEY.	210



167.

LIVERPOOL AND MANCHESTER

MEDICAL AND SURGICAL REPORTS.



ON PAST AND PRESENT TREATMENT OF INTES-
TINAL OBSTRUCTIONS.

By HUGH OWEN THOMAS.

FROM the history of these affections I have culled some of the opinions held regarding them and their treatment by the ancient practitioners of medicine.

Hippocrates refers to ileus and its treatment, but to my mind it is very apparent he was not able to distinguish between a loaded rectum and what our great modern authority, Dr. Brinton, would affirm to be ileus. Yet, it is evident that when he treated a case of genuine ileus his treatment was not so far astray as that of some of his successors, as is apparent from his mentioning the seventh day as being usually the limit of the patient's endurance.

His treatment consisted principally of inflating the intestines, and the administration of enemata. The same treatment seems to have been practised by Celsus, Paulus Ægineta, and Aretæus. The last, though he says that "In ileus it is pain that kills, along with inflammation of the bowels, straining and swelling;" yet, he informs us that no "respectable physician" would administer a narcotic except in extreme cases.

Until we come to a date so late as the 17th century, this complaint seems to have been looked upon as nearly always fatal. The father of English medicine, Dr. Sydenham, appears however to have been in advance of many physicians living even in our own times. He appears to have used opium freely, and with marked success in intestinal irritation and obstructions, much in the same way as the late Dr. Brinton would have practised, but with this exception, that the former, on most occasions, commenced with a sharp purgative, for the purpose, as he says, of clearing the way for the action of other remedies. One original remedy he refers to, and reports highly of, but which Dr. Brinton very properly ignored, is the application of a live kitten to the stomach during the continuation of any vomiting, insisting upon the necessity of not removing this pleasant companion till the vomiting should cease. As a contrast to this harmless advice, and indicative of Sydenham's careful clinical observation, I quote his remarks on the value of enemata. In his Medical observations he says :—

“Hence I have more than once remarked that the repetition of even the ‘mildest glyster has induced serious symptoms.”

Here he not only anticipates all modern teachers, without exception, but also surpasses them in this one item of rational treatment. My own experience is that the administration of a single enema of water only may give rise to serious symptoms. On two occasions in my own practice, serious symptoms occurred; though the water given as an enema in one case did not exceed four ounces, in the other twenty; both cases were fatal; yet did well up to this injurious and unnecessary interference.

He records that in the month of August, 1671 :—

“The most noble Baron Annesly, who was suffering some days from bilious colic, accompanied with intolerable pain and a frequent desire to vomit, sent for me to see him at Belvoir Castle. He had already tried two kinds of Glysters and other remedies to boot, and these had been ordered him by the most learned medical men of the parts around. I made no difficulty in prescribing the repeated application of narcotics after the plan described, by the use whereof he mended every day, and returned along with me to London a healthy man.”

He records another case :—

“ A poor neighbour of my own, who is still living, was during the years in question afflicted with a most violent bilious colic, which he had tried in vain to subdue by purges, glysters and swallowing leaden bullets. I ordered him to use narcotics frequently; this he did, and as often as he did so, found himself relieved. The disease however, was only palliated, not cured. It returned when the virtue of the anodyne was spent.”

The author records that this case ultimately recovered by this treatment.

Dr. Sydenham's method, despite his determination at the commencement of treatment to get rid of “peccant humour” by purging, and the application of the Sydenham placebo, a live kitten, appears to have been the best treatment up to the advent of Dr. Brinton's demonstrations. But I have reason to believe that this treatment fell into disuse among practitioners immediately after his time, probably he would be the only one of his contemporaries that practised the opium method, for we find some of them, as for example, James Cook, Surgeon, commenting on the treatment of ileus, stating that, “narcotics have no place in colic;” the specifics advised were composed of the dried viscera and appendices of animals, powdered and mixed in sack. Probably Mr. Cook would be one of those gentlemen to whom Sydenham refers as “learned men of the parts around.”

After making diligent search among the records of cases reported during the 18th century, and the early part of the present century, I have failed to find any account of cases of intestinal obstruction treated by Sydenham's method, or any approach to it. Among those I have examined were the ‘*Medical and Physical Journal*,’ the ‘*Lancet*,’ ‘*The Medical Times*,’ ‘*The British Medical Journal*,’ and some other medical periodicals; but I have not been able to find a single case of intestinal obstruction, no matter from what cause, treated by the administration of opium, combined with the utmost possible prolonged rest of the parts affected; and the discussion of this subject by the Royal Medico-Chirurgical Society of London, reported in the ‘*Lancet*’ of the 18th December, 1875, apprized me for the first time that the rationale of the treatment of this difficulty by opium was not known even to leaders in the profession. From the report of that discussion, as given in

the '*Lancet*,' it is obvious that the treatment of Intestinal obstructions has retrograded since the death of Dr. Brinton, who evidently was the first and, up to this date, the best authority for our guidance.

Dr. Brinton's volume on Intestinal Obstructions is undoubtedly not so well known as it should be among the profession; and it is my opinion that no subject, out of mathematics, has ever been so thoroughly and exhaustively demonstrated as the one to which Dr. Brinton's work is directed; and I have no hesitation in saying that if all the now debatable questions in Surgery and Medicine could be so well illustrated as this subject has been by him, Medical art could make claim to a place amongst the exact sciences; and yet, though the author promulgated his views in the *Croonian* Lectures, delivered before the College of Physicians in 1867, it is shewn both by the discussion before referred to, and the reports of our weekly Medical periodicals, that the treatment of Intestinal Obstructions has not advanced from where it was previous to his researches.

By the death of this talented physician, which took place while the re-publication of his lectures was in progress, the medical department of our art lost an able and most persevering demonstrator of a rational and successful method of treating these cases. It is not a matter of surprise to me, that in the discussion before the Royal Medico-Chirurgical Society, some gentlemen doubted the correctness of Dr. Brinton's statistics. This may be accounted for by the fact that the method of treatment which he demonstrated as being necessary to increase the probability of success has been generally ignored, which has certainly more than trebled the mortality.

In the volume for 1853, of the '*British Medical Journal*,' page 433, Mr. Joseph Hinton gives a table of cases of Intestinal Obstructions, with a brief outline history of 137 cases, inclusive of herniæ. Over 100 of these were cases of intussusception, volvulus and enteritis; the remainder were herniæ, malignant tumours, and stricture; yet, in not one of these cases was an exclusively opium treatment tried.

Some of the cases are recorded as having been treated with

opium and bleeding, but on reference to the source of information I notice that calomel and enemata were also used.

I refer to Mr. Hinton's table as being both instructive and interesting to those having a special interest in the subject.

In order to illustrate how little Brinton's discoveries have influenced the treatment of this disease, I append a condensed report of a case long anterior to his time, and recorded in the '*Medical Physical Journal*,' 1824, page 116:—

Enteritis.—The treatment commenced on the

First day—Bleeding, purgative, enema and jalap powder.

Second day—An enema of tobacco water.

Third day—Castor oil, opium and purgative draught.

Fourth day—Purgative draught.

Fifth day—Bleeding.

Sixth day—Anodyne enema.

Seventh day—Laudanum injection, bleeding, and calomel.

Eighth day—Bleeding, cold applications, calomel, common salts, senna, tobacco injection.

Ninth day—Tobacco injection, enema of salts and senna, calomel and antimony.

Tenth day—Doses of calomel, antimony, castor oil, extract of hop.

Eleventh day—Castor oil only.

Twelfth day—Purgative draught.

Thirteenth day—Purgative draught and apple dumplings.

Fourteenth day—Sore in the mouth, but convalescent.

The reporter of this case states in page 120 of the same volume, that the purgatives were sparingly used. What must a liberal system of purgation have been in those days? With the treatment above set out, wonderful to relate, the patient recovered. There was no action of the bowels until the fourteenth day; in fact, not until the probable period of resolution.

Another sample of the treatment adopted about that period is reported in the '*Lancet*,' vol. 16, p. 415. The patient was suffering from supposed enteritis, and he was treated with hot fomentations, five grains of calomel, and a purgative on the first day, and this was repeated on the second day. On the third day there was a repetition of this treatment, with an addition of an enema night and morning. Fourth day—calomel, colocynth, castor oil, enemata. On the Fifth day a change of tactics took place. The

body was immersed in warm water, and the abdomen rubbed with oil, followed by the administration of croton and castor oils. On the sixth day a powerful purgative, and an enema were given. On the seventh day, croton and castor oils with enema of aloes. On the eighth day another change of front took place; scammony, gamboge, senna, magnesia, and tincture of rubarb, were freely administered; and on the ninth day, croton oil. After the administration of which we are informed that "despite the treatment" "the patient sank at 10 o'clock." Two hours before his death, an anodyne was given. This is an illustration of the treatment in 1828.

These two reports may be compared with what is done in our own time; if we take for example a case reported in a number of the '*Lancet*,' published during 1874, containing the details of a case where the patient on the first day of his admission to a public hospital, suffering from what was supposed to be some form of Intestinal Obstruction, the exact cause it was not possible to diagnose. To commence treatment the patient was ordered one drop of croton oil, ten grains of colocynth, and three ounces of brandy. On the second day, extract of belladonna and linseed oil enemata were made use of. On the third day, belladonna, galvanism by contact through the rectum, and the abdominal wall. On the fourth day, belladonna and brandy. On the fifth day, the patient died.

The post mortem examination shewed that had the treatment been more rational, it might have prolonged the life, but would probably not have cured this patient; though Brinton asserts that recovery is not impossible in the condition present in this case by the opium treatment.

Again in the '*Lancet*' for the present year, a case of Intussusception of the large intestine is reported. The treatment adopted was copious warm soap and water injections. The case was fatal, the post mortem examination shewing that an advanced degree of recovery had taken place, and that had Dr. Brinton's principles been thoroughly carried out in practice there was a great probability that the patient might have recovered.

In the same volume, in the column devoted to correspondents,

another case is mentioned, the treatment of which was commenced with an enema of castor oil and turpentine, with an "internal compound to stimulate the intestines," the enema being repeated while the patient was sinking.

It may probably be fresh in the memory of the reader, the report given in the "*British Medical Association Journal*" of the treatment adopted in the case of the late Madame Du Devant (better known as Georges Sand). In her case, evidently one of the élite of the profession was invited from Paris to her Chateau, near Mohant, to assist "the learned men of the parts around" as to the treatment to be adopted, with the result, it appears to me, of a repetition of that treatment which some of the contributors to the '*Medical Physical Journal*' of 1824 would have advised. In fact, the patient's chance of recovery would have been better had she had no advice at all, rather than the injurious interference to which she was subjected.

I will take another example from a recent number of the '*Dublin Journal of Medical Science*,' in which a case of Intestinal Obstruction is recorded, which was treated on the first day with enemata of "various kinds," "purgatives of different sorts," "including castor oil, scammony, calomel, and croton oil." This treatment was continued for several days, when a change of plan seems to have been decided upon, and extract of opium was given by the mouth every fourth hour. On the sixth day a return was made to the previous treatment with purgatives; rubbing the bowels with warm oil had been constantly persevered with during intervals of the administration of medicines. Sometime between the 6th and 9th day the distended abdomen was relieved of gas by puncture, and the opium treatment was again resorted to; then about the 10th day, galvanism was applied. On the 15th day, castor oil and rhubarb were administered, with the result of producing a return of most of the symptoms which had begun to abate. This case, wonderful to relate, survived the treatment.

From the foregoing examples and others which have come under my notice during the last ten years, I am convinced that there are very few in the profession who are acquainted with Brinton's labours, and fewer acquainted with the correct treatment

of this disease, and many who are cognisant of his views want the confidence to apply them undeviatingly in practice.

Having myself, in my earlier practice, previous to my study of Brinton's work, used the methods of purgation, enemata (though never in the heroic manner that I have noticed in others) I can confidently say that the opium method involves as great a probability of success, as the other methods increase the improbability of the patient's recovery.

The '*British Medical Journal*,' of July last, contains the report of a very instructive case of intestinal lesion, partially treated by opium, as on perusing it I notice that the treatment was commenced by calomel, opium, and enemata on the first day, the enemata being repeated twice the same day, and the latter also being repeated on the day the practitioner judged the patient had been relieved of the difficulty.

This case is the nearest approach to Dr. Sydenham's method that I have met with amongst reported cases, and very strongly confirms the opinion that I have formed during my own experience, that in the majority of these cases the administration of opium by the month is of little avail, having but slight control over the complaint. Judging from the symptoms recorded, I should class this case as one of enteritis progressing to resolution unaffected by remedies. I have frequently met with cases in which, from some cause, opium given by the month in large medicinal doses seemed to have none of its usual physiological or therapeutical effects.

These examples indicate that the treatment in the early part of the present century differs in no way from that of our time, except that we have added to the treatment another objectionable feature—Galvanism. It may be interesting to notice the practice in use twenty years ago, but it seems to vary but little from the practice in vogue in the present year.

This can be further shown by a comparison of the paper contained in the '*British Medical Association Journal*' for 1853, p. 117, where is recorded a series of nine cases. I append a condensed account of each. The first case was treated by opiates and enemata, with metallic mercury, and the patient took 7 lbs. of this metal and yet recovered.

The second case was treated by calomel, colocynth, black draught, castor oil, enemata, turpentine stupes, and a pint of newly fermented yeast, and recovered.

The third case was treated by calomel, opium, castor oil, and enemata, and proved fatal, the patient succumbing in 12 hours.

The fourth case was treated with purgatives, and died on the third day.

In the fifth case nearly all the list of purgatives was tried, also quicksilver and tobacco enemata, and the patient died on the sixth day.

The sixth case was treated by purgatives, and proved fatal in 13 hours after the commencement of the attack.

The seventh case was treated with purgatives and tobacco enemata, and also proved fatal.

The eighth case was treated with purgatives, and opium, and proved fatal.

The ninth case was treated with mild aperients, opium, and enemata, and was fatal on the third day.

In comparing the treatment of these cases with that of one reported and discussed before the Clinical Society of London so late as last October, and reported in the '*Lancet*,' of the 21st of the same month, I am forced to the conclusion that we are not improving upon the treatment practised in times gone by, but rather, retrograding. This case, the details of which were discussed before the Clinical Society, appears to have been diagnosed as one of intestinal obstruction, and yet the details of treatment were, daily enemata, hot fomentations, castor oil, croton oil, and turpentine; the passage of a long tube up the bowels, inverting the patient and shaking him in the inverted position, trocaring the bowels, kneading and manipulating the abdomen, galvanism (with the intention, it is reported, of exciting peristaltic action), the administration of extract of aloes, and a combination of enemata and kneading; and still more remarkable, it is reported that death occurred suddenly and unexpectedly on the 59th day, after all this heroic treatment. Surely, death could only be expected, as, to all the remedies so trying to the patient's powers of endurance, was superadded a very serious complaint.

In no recorded case but this can I recollect such a systematic negation of the principles which should guide us in the treatment of such cases, and it is certainly notable that in the discussion which followed the reading of the report, none of those engaged in it are reported as having dissented from the mode of treatment, although the editor of Dr. Brinton's volume took part in the discussion. It appears to me that Dr. Brinton's valuable demonstration, as far as its practice is concerned, has been buried with him. This patient's remarkable power of endurance both of the treatment, and complaint, terminated at the end of 59 days. I should judge from the report, that, under a more rational treatment, even if ultimate recovery was not possible, life could have been prolonged three times that period. Dr. Brinton asserts:—"With
"all the resources of medicine directed to the sustenance of life,
"and in a constitution of exceptional tenacity, I am persuaded that
"a duration of at least 4 months would be attainable." He further adds that, "the treatment hitherto practised has trebled the rapidity
"of the malady." It is astonishing what control opium has over the pain, vomiting, and other symptoms which usually accompany these difficulties, even when, as in this case, ultimate recovery is not probable. In confirmation of my assertion as to how imperfectly the principles of treatment are understood in the present time, I subjoin a quotation from Dr. J. S. Bristowe's recent volume on the Theory and Practice of Medicine, published last year, advising treatment for intussusception, page 728.

"In those cases, however, in which the symptoms of obstruction come on
"vaguely and without evidence of association with inflammatory mischief, it is
"generally advisable to commence the treatment with the administration, either
"by the mouth or rectum, of moderately powerful purgatives, and to persist in
"this treatment until, by their failure to act, and by their causing vomiting and
"painful but fruitless peristaltic movements, their inefficacy is distinctly shown.
"It sometimes happens that, after drastic purgatives have failed, a large dose of
"some simple laxative, such as castor oil, acts with singular efficacy. In aid
"of this treatment, hot baths, fomentations, or ice or electricity to the surface
"of the belly, and voluminous enemata of gruel or of water may severally be
"employed. If those measures are without avail, it is generally advisable to give
"the bowels rest, and to relieve pain by the repeated use of adequate doses of
"opium or of belladonna; the persistence in which treatment will, by relieving

"spasm, or otherwise promoting the return of some length of bowel to a comparatively healthy condition, not unfrequently result, after a shorter or longer time, in an effectual and sufficient evacuation. If this treatment fail in its turn, it may be necessary again to solicit the action of the bowels by the employment of purgative medicines, enemata, and the like. Such is the routine which must be generally followed in cases of simple obstruction, in which the cause of obstruction is obscure; and in many cases also even when the cause is distinctly ascertained."

Here we are advised to commence with powerful purgatives, and to persist in their use until we have evidence of their injurious action, then mild laxatives can be tried, aiding all these by hot fomentations, electricity, and enemata, failing in all these opium or belladonna is to be given; after which latter, if they fail, a return to purgative and enemata, &c., is counselled. In fact, it may be noticed that there is, in the treatment recommended, an utter absence of any systematic method based on the etiology of the difficulty under consideration. These lesions are of such serious import to life that it were better to practice an expectant method than to incur any risk by giving remedies not based on successful clinical or experimental observation. Many cases have been reported as having recovered even after the most inefficient treatment, which to my mind is strong evidence that with a more rational treatment the mortality would be decreased.

As an instance of remarkable recovery from intussusception, there is recorded in volume 16 of the '*Lancet*,' p. 7, a case in which three feet of intestine, with a portion of its mesentery attached, came away. This case is reported by a Professor of Anatomy, a guarantee that it was intestine that was passed.

In volume 11, p. 565, Mr. Abernethy reports a case where a portion of the intestine sloughed and came away. The treatment is not given in either of the above cases.

In the transactions of the '*British Medical Provincial Association*,' 7th volume, a case is reported of recovery after five inches of intestine had passed, though treated by the purgation method.

My friend, Dr. Turnour, of Denbigh, informed me that he had a case where a large portion of intestine sloughed and came away;

his treatment being the administration of opium, the use of which he strongly advocates in these lesions. Dr. Bristowe, in his recent volume, reports on the authority of Dr. Peacock, of London, a case in which the sufferer passed twelve feet of gut, and recovered. This extraordinary and unprecedented report induced in me some doubt of its correctness. I communicated with Dr. Peacock, who very readily favoured me with a reprint of his paper "A case of invagination of the intestines followed by the passage of a large piece of bowel by the rectum," originally published in Transactions of the Pathological Society, vol. xv. From a perusal of his paper I find that instead of twelve feet the portion passed measured only thirty-five inches. The author gives us a short history of twenty cases of invagination with sloughing of portion of intestine. Case 18 is reported on the authority of Drs. Harley and Bristowe, as having passed the almost incredible length of four feet, with recovery; the period of separation varying from the sixth to the thirtieth day. One case is mentioned in which "portions were passed at intervals during a period of three years." In my opinion, this was a case of chronic catarrh of the bowels, attended with the occasional passage of casts of the intestines. In case 10, where four feet of gut is reported to have passed per rectum, this might also have been a large cast of the bowel; but as the previous history of the patient is not given, it is not possible to form a decided opinion. My own doubts arise from the report, which describes the portion when examined "as the mucous membrane not the whole of the bowel."

As to the causes of Intestinal Obstructions, the immediate ones are generally peritonitis, enteritis, paralysis of the muscular fibres of the bowel from contiguous inflammation, occurring in the condition of peritonitis, enteritis, or after herniæ reduction, or operation. Paralysis also may arise from over distension, from compression during and previous to reduction of, or operation for herniæ, invagination, rents in mesentery, twisting of the gut, stricture, and malignant growths. It has too frequently hitherto been thought that obstruction arising from herniæ, is practically on a safe way to resolution after reduction or operation, when in my opinion the reduction or operation frequently only places the

patient in a condition fit for rational therapeutic treatment, and he should be treated by the opium method, and dieted as strictly after reduction or operation as though it were the first day of any of the varieties of obstruction before enumerated.

Some writers mention spasm, accompanying enteritis and lead colic, &c., as a cause of obstruction in some cases. My own conviction is, that spasm is never a cause of obstruction, and if its occurrence is granted, its presence in enteritis is impossible, as the inflammation would temporarily suspend muscular action. Our knowledge of the etiology of lead colic does not as yet justify any one in asserting or denying the occurrence of spasm in this condition. Tobacco has usually been judged a valuable and efficient remedy in this disease; if this is correct, it rather negatives than confirms the spasm theory, inasmuch as tobacco is a stimulant of the muscular coat of the intestine. The treatment of lead colic is no exception to that of any of the other varieties of intestinal lesions. I have always, in these cases, followed a mode of treatment exactly the same as that here advocated in Intestinal Obstruction, with unvarying success. My appointment as Medical Officer to a benefit Society, composed exclusively of painters, has given me a few extra opportunities of noticing the effect of opium in these cases, during the last seven years. At one time I tried alum in lead colic, a remedy much advocated by the late Dr. Inman, but failed to find that it had any effect in the disease, more than an expectant method would have.

A diagnosis of the existence of intestinal irritation or obstruction is not always easy at its onset, and may easily, and in my opinion often does, escape notice; especially a slight or chronic condition of peritonitis or enteritis, which is often considered as merely general malaise, and so not subjected to special treatment which would avert a more serious condition at a later period.

The subjective symptoms usually present at the onset of intestinal complication are a feeling of fulness, weight, and tightness of the abdomen, as though its contents were heavier than usual, a slight thirst, headache, and vomiting more or less frequently. These, if not relieved, are followed by the objective symptoms: tenderness on pressure, perceptible distension, at first gaseous, and

then fluid and gas. They are the general symptoms present in the majority of cases. These may become, intensified by injurious, continued from neglect, or diminished by appropriate, treatment. As for instance where purgatives are administered they aggravate the above symptoms without one exception, and prolong the recovery of the patient, or ensure his dissolution at an earlier date than would have been the case had he been neglected, or appropriately treated.

Neglect is attended with symptoms of lesser severity than those present during inappropriate treatment, while with appropriate treatment all the symptoms become much diminished in intensity and frequency, and the patient often has such relief that without a knowledge of his history and an examination of the abdomen, the practitioner would not believe him to be a subject of suffering. It is certainly easy, with ordinary care, to diagnose even at the onset the coming intestinal difficulties, but the attainment of a differential diagnosis is difficult. Here I am conscious of my own deficiencies, and that also the same defect is only too apparent in the writings and teaching of others.

Those who have studied the records of reported cases will probably have noticed that a differential diagnosis is generally arrived at after some days of treatment and careful watching of symptoms; as for instance in peritonitis and enteritis, where we have principally gaseous distension with evacuations at an early date, and a gradual relief of distension. (I am supposing the opium treatment is practised.) In invagination there is a longer period of constipation, nearly normal temperature and pulse, also the abdominal distension is mostly fluid and a sudden relief of distension occurs. In the various forms of constricted gut and volvulus, collapse occurs at a time corresponding in period to that of herniæ collapse, and indicating that the operation of gastrotomy is advisable. It may be broadly stated that a differential diagnosis is easier guessed than demonstrated.

Under these circumstances it is a source of satisfaction to know that a differential diagnosis is not necessary to successful treatment. It may be asserted, as a general rule, that the treatment is similar for all the varieties of intestinal complications.

The mechanical remedies usually employed in the treatment of these obstructions are, medicated enemata, inflation with air or carbonic acid gas, the administration of liquid mercury, the passage of O' Byrne's tube, the inversion of the patient and shaking him; and one of our medical periodicals lately reported the administrations of copious enemata of warm water whilst the patient was in the inverted position. Trocaring the intestine, galvanism, kneading, external applications, and gastrotomy, may be classed among mechanical remedies in intestinal obstruction. To these I may also add a remedy which I claim as peculiarly my own, namely, that of retaining the patient upon an inclined plane, the shoulders and head being dependent, and the pelvis elevated; this position I have observed to be of decided advantage in severe cases.

It is with some diffidence that I differ from so high an authority as the late Dr. Brinton with regard to the value of enemata in these cases, whether they be administered for the purpose of diagnosis, or supplying nourishment, or giving sedative medicine, or for mechanical purposes, even should they be but two or three ounces in bulk they may again cause the recurrence of all the symptoms that the sedative medicine may have controlled. In the treatment of these affections they should, in my opinion, be totally discarded in every form and for all purposes; whether the bowel below the obstruction, which it seldom is, be loaded or not. Should there be some accumulation below the obstruction this accumulation can do no harm. Most of my readers will agree with me that it is the accumulation above—not below the obstructed part, which constitutes the difficulty; and interference with the contents below, readily excites peristaltic action, even in the parts above the obstruction, and adds to the practitioner's difficulties in successfully treating the case. I have observed that the passage into the rectum of even a soap suppository, to cause a return of symptoms that had been controlled. Indeed the call for nutritive enemata is never so urgent in these cases as to justify this practice, pain is the principal cause of emaciation and death, not starvation, and it will, I think, be granted that with the suitable forms of aliment now so readily supplied to us by art, and with opium to

allay the pain and its sympathetic irritation, it would take a very long period to seriously emaciate the patient. On this head there should be no anxiety, as we know that from 14 to 60 days is the usual period of recovery or death, which results from the lesion, but never from emaciation; and it may be laid down as an axiom, that the more the patient abstains from imbibition and aliment (though suitable,) the sooner his recovery and consequently a shorter period of abstinence and return to usual routine of diet will be attained.

Dr. Brinton has very properly remarked :—

“Nature herself is preparing within the obstructed bowels the best of all “purgatives, the most admirably adapted by its quantity and quality—and especially its consistence, to accomplish whatever an aperient can do towards “opening a passage.”

My own opinion, based upon my observation in practice is, that it should be the study of the practitioner to restrain the action of the bowels so long as possible, as by so doing he decreases the consistency of the abdominal contents, and so makes more safe, certain, and gentle, the action of the aperient when the time arrives; when prolonging the constipation is no longer possible. For myself, I have never noticed, where constipation has been well maintained by opium beyond the 14th day, the patient pass solid *feces*; it has always been liquid or pultaceous.

As to the proceeding by inflation it appears to me a useless torture. With regard to the value of “*Crude Mercury*,” this can only accumulate at the obstructed point, and is, by its weight more difficult of expulsion than the accumulated liquid already there; and this the patient has to relieve himself of by occasional vomiting. Sydenham thus comments on the value of this remedy—

“Whatever may be said about bullets and quicksilver; things which, “whilst they can effect but little good, can do much harm.”

The passing up the rectum far into the colon of O’Byrne’s tube, I cannot and never could see its purpose, not even in a loaded and over distended, consequently a temporarily paralyzed rectum. As a general rule, in the latter condition it is a spoon or spatula that is wanted.

Inversion of the patient has been practiced frequently of late in supposed intussusception. This, to have any chance of success,

must be practiced early, within a few hours, and previous to agglutination between the intussuscepted parts; otherwise, as has been demonstrated, it may be impossible to reduce the intussusception even when the bowels are under direct manipulation. Inversion with shaking, if practiced at a late period, should it succeed in reducing the invagination, the result might be calamitous to the patient, as agglutination and absorption with or without sphacelus may have taken place; and to confirm this opinion, I refer the reader to an early number of the '*Lancet*,' for 1876, in which is reported a case of intussusception where gastrotomy was performed, yet the invagination could not be reduced immediately after operation, or afterwards at the post-mortem; in fact, the case was in progress to spontaneous cure had it not been operated on. In the 12th volume of the '*Lancet*,' page 580, Charles Bell, in a lecture on Intussusception, emphatically objects to mechanical interference, as advocated by Hunter. The following are his words:—

"At a certain period, if the contraction were fairly in your hand you cannot undo it.... Fixed by inflammation, and then no power, I believe, that you have will undo it."

Sydenham and Charles Bell appear to give correct explanations to several questions of the etiology of this subject, anticipating Dr. Brinton; as, for example, Sydenham explains the mechanism of vomiting; and again, Charles Bell lucidly instructs us as to the mode of formation of, and subsequent changes in invagination. Again, the direction of the invagination is to be considered, but it is not possible to diagnose this; consequently, the inversion of the sufferer may increase the difficulty; shaking and injecting water into the intestine while the patient is inverted, is open to the same objection as inversion alone is.

Kneading the abdomen, perchance, might do something; but in all probability, that something will be injurious. Of all the mechanical methods, none in practice has appeared to me so decidedly beneficial as confining the patient to the inclined position, with pelvis raised and trunk dependent. It diminishes distension by facilitating a more copious vomit, which is effected

with much less effort, at the opportune period. I have assumed here that the patient is under the effect of opium, given sub-cutaneously, and has the periodical vomit only, indicating the effect of opium. This inclining of the couch I usually obtain by placing two or three claybricks under the two posts at the foot of the bedstead.

The next mechanical addition to modern treatment has been to trocar the distended gut. This, though in my opinion not essential to success at all times, is a means of temporary relief, and has to be employed with care, as some surgeons have observed post-mortem indications of leakage from the punctured bowel. In my judgment, this may have arisen from carelessness in the mode of withdrawing the trocar.*

To avoid extravasation of stercoraceous fluid into the peritoneal cavity after puncture, it is advisable, before withdrawing the canula, to cleanse its external orifice, then to place the thumb on the orifice, and secure counter-pressure with the fingers around the canula. The instrument is thus plugged so as to ensure that a rush of air does not empty the canula into the peritoneum during its withdrawal, as is done when we wish to avoid spilling the contents of a catheter, while withdrawing it from the bladder. There is one condition in which it appears to me the operation of trocaring may be specially indicated: viz., when the obstruction occupies the descending, transverse, or sigmoid colon, and the accumulation has extended up to, and is straining the, ilio-cæcal valve. Here the use of the trocar may relieve pain, which may require a very large dose of opium to control, and avert gangrene, or rupture, of the intestine. It may be stated as a rule, based on published post-mortem records, and which my own experience inclines me to believe, that while trocaring the intestines to empty them of flatus is perfectly safe, the removal of their liquid contents may not be so free from danger.

* Messrs. Krohne and Sesemann have made a very suitable and effective set of trocars for this purpose, from my design. They are so arranged that the canula is within the needle; this is for the purpose of avoiding a shoulder on the needle, sometimes an obstacle to its entrance into the bowels; the canula also converts the lance point into a blunt one after its entrance into the bowels.

In the category of mechanical aids to the treatment of this difficulty, I have placed Gastrotomy and Enterotomy. One of these operations is, in my opinion, at times urgently indicated. Gastrotomy, if performed early, has a probability of success; but then it would have to be performed at a very early period, when medical treatment could not therefore have been fairly tried; and we know that, in most cases, therapeutical means do succeed, and involve in their practice a fraction only of the risk attendant on this serious operation—gastrotomy. Again, supposing the operation to be successful, the patient even then has to be subjected to an efficient method of restricted diet and therapeutical remedies, in the after treatment. To perform gastrotomy in a case of obstruction from peritonitis, enteritis, or invagination, few surgeons would, I expect, advise, as these are efficiently treated by medicine. Indeed, its performance, where the difficulty arises from intussusception would be purposeless after the first few hours of invagination; as in all probability the intussusception could not be reduced if found; consequently, this operation would seriously compromise the chance of the patient's recovery. Past and recent records of operative interference by gastrotomy fully confirm this view. Performance even at an early stage in obstruction from intussusception would be a questionable practice, knowing, as we do, from records how much nature has done in these cases, even when retarded by misdirected art; much more success may we expect from improved treatment in this special class of cases. Dr. Brinton expresses his opinion thus, "An operation ought not, I think, to be mooted." Again, he gives his opinion that after a certain time operation in this condition "would at the same time literally withdraw the patient's only chance of recovery." In my opinion, the operation of enterotomy involves a much greater probability of success than that of gastrotomy; because, first, after successful operation there is no occasion for special diet or therapeutical treatment, as the obstruction can not possibly continue if the gut is opened above it; or, if sphacelus has occurred, this should be the part of the gut operated upon, and should be attached to the external abdominal opening. Secondly, should

any of the varieties of obstruction be complicated by adhesions, &c., there is no occasion to interfere with any progress towards recovery that may have taken place, as enterotomy relieves distension and makes reaccumulation improbable, if not impossible, so long as no new obstruction arises in any part of the intestine higher up than the part already operated upon. The cases which here follow confirm me in the opinion that the operation of enterotomy should be preferred, and performed only, when symptoms of threatening, or of actual, collapse appear. It would be better to wait until these symptoms occur, than add to the records of cases operated upon to find that the patient might have recovered, if he had not been interfered with by operation.

In the '*American Journal of Medical Sciences*,' the reader will find a collection of 13 cases of abdominal section for supposed intussusception, compiled by John Ashurst, M.D., which article is a serious indictment of the modern basis upon which this operation is undertaken. Dr. Ashurst also gives a table of 57 cases operated on for other causes (not intussusception), but as very imperfect details are given, it is not possible to glean any practical information from them. Of the 13 cases of operation for intussusception, two are of uncertain details, and may very properly be excluded; the remaining 11 cases are recorded with sufficient data, that it is apparent to me they were all, previous to operation, subjected to a preliminary treatment such as Dr. Sydenham would, and Dr. Brinton did, designate as the "grossest malpraxis." Appended here are the data pertaining to each case:—

No. 3, Male, 50 years.—Primary treatment—enemata, opium, calomel, laxatives, soap linaments, bougie up rectum, warm bath, tobacco enema, and finally the operation of abdominal section. Twelve inches of adherent invagination, &c., was found, and the case ended fatally next day.

Post-mortem revealed a gangrenous condition of the bowel involved, consequently this operation in this case must at least have been detrimental.

No. 4. Male, 28 years.—Primary treatment—cathartics, enemata, bleeding, anodynes, linaments, baths, and metallic mercury. Tenth day—abdominal section—successful reduction of nearly two feet of invaginated gut. Patient recovered.

No. 5. Male, 12 weeks old.—Primary treatment not given. Operation of abdominal section, bowel adherent and gangrenous; also ruptured by the attempt at reduction of invagination. In progress to recovery—finally fatal.

This case must also be placed among those, calamitous from unnecessary interference, enterotomy could have succeeded.

No. 6. Male, 20 years old.—Seventeen days obstruction. Primary treatment, purgatives and metallic mercury; finally, abdominal section; invagination of ileum was found. "Disinvagination was effected by grasping the intestine above and below, and forcibly rupturing the adhesions, which were quite firm; the bowel and omentum were deeply congested and on the verge of mortification. This patient recovered."

Wonderful recovery when we consider the primary treatment and unnecessary and detrimental interference by operation with the part which was in progress to resolution.

No. 7. Male, 36 years.—Primary treatment, leeches, liniments, enemata—cold and hot, abdominal section. Unsuccessful as regards reduction of invagination, and fatal.

No. 8. Child, 4 months old.—Duration of complaint, 4 days. Primary treatment, injection, insufflation, sponge probang, then abdominal section. Disinvagination was effected with some difficulty; which latter indicates some degree of progress to recovery. Child was almost moribund just before operation, and died five hours after its termination.

Primary mechanical treatment had exhausted the vitality of this subject so that he cannot be said to have been further injured by operation.

No. 9. Male, 16 years old.—Primary treatment not given. Operation—bowel gangrenous—no reduction of invagination—bowel opened and formation of artificial anus.

Patient died shortly after operation. Case for enterotomy.

No. 10. Age not given. Treatment not given. "Disinvagination was impossible on account of the existence of adhesions."

The case terminated fatally. From the report it can be fairly supposed that this case was also in progress to recovery, had it not been interfered with by section.

No. 11. Details not given, but mentioned as fatal.

No. 12. Female, 6 months. Primary treatment not given. Operation first day. "Disinvagination was effected with great difficulty."

This means that adhesions existed, and it may be inferred that this case again was in a fair way of recovery, had it not been disturbed by operative interference.

No. 13. Case 2 years old. Intussusception. One month in formation. No treatment given. Operation successful.

To these cases of Gastrotomy, collected by Dr. Ashurst, I now add one of Enterotomy, reported by Dr. A. Brigham, Superintendent of the New York Lunatic Asylum, and reprinted in the *American Journal of Medical Science*, for April, 1845—page 355. This case may be described as one of Enterotomy, without primary interference, and is a remarkable example, both of recovery and of the mode adopted by nature to secure the resolution of so extensive a mutilation as the removal of seventeen inches of intestine is.

"The following very remarkable case of recovery, after extensive loss of small intestine, has lately occurred. The patient, a married woman, who had had five children during the two previous years, was admitted into the Asylum with which Dr. Brigham is connected in June, 1843.

As some fears were entertained that she would attempt suicide, she was strictly watched, and placed in a room where no instruments by which such purpose could be effected, were kept.

"No material alteration in her case was noticed until Oct. 24th, when about nine o'clock in the forenoon, she obtained a pair of large scissors, that had been accidentally left in the hall, which she took to her room and with which she made two wounds into her abdomen, one about an inch and a half above the umbilicus, the other half an inch below it. From the upper opening she took out part of the small intestines, from which she cut off a portion, seventeen inches in length, when she was discovered by another patient, and alarm being given, she was forced, not without some resistance on her part, to cease from further injuring herself.

"Dr. Buttolph, the assistant physician, was near, and saw her immediately, and discovering that the intestine was entirely separated, and also a considerable portion of the omentum, and that one end of the intestine was withdrawn into the abdomen, concluded the case would soon prove fatal under any treatment, and therefore returned the end of the intestine that protruded into the abdomen, stitched up the wounds carefully, and covered them with adhesive plaster,—applied a bandage around the body, gave her an attendant to remain with her constantly. While thus dressing the wound she vomited, but did not appear to have much pain.

"On examining the detached intestine, which she had cut into in several places, it was found to contain a small quantity of fæces, and weighed one ounce and one drachm; the omentum, which was separated from it, weighed one ounce and two drachms. The ends of the intestine were ragged, and had been cut off obliquely. For a few days she was disposed to vomit, and was not able to retain any thing on her stomach but a trifle of water. Injections of laudanum and broth were administered, and she was kept constantly quiet. After a few days she called for food, and was able to retain a very little, and in about ten days she asked 'if she had not ought to take some physic?' She was reminded of the accident, and told that it would be improper to give her physic; but she did not appear to think so, and said she 'felt as if it would do her good, and that she ought to have some.'

"She continued without much change, very quiet by aids of injections of laudanum, eating a little several times in the day, and vomiting occasionally, but without any marked tenderness or inflammation of the abdomen, until the 26th of November, thirty-three days after the accident, when she had a small discharge from the bowels of hardened fæces, and on the next day a copious one. This, she said, gave her great relief, and from this time she began to improve. The wounds had already healed, and she was soon able to walk about. Since then she has continued to have regular evacuations from the bowels, though there is rather a tendency to diarrhoea, for which she often takes laudanum. She now eats tolerably well, though inclined to vomit when she eats heartily. She is able to be about the house, and sews and knits, and is as well as she was for several weeks previous to the injury. She is, however, still feeble, and does not gain flesh, but is calm and quiet, though her mind is in rather a demented state."

Dr. Brigham, in the '*American Journal of Medical Science*,' for January, 1846, page 44, reported the death and post-mortem examination of this patient in 12 months after from some disease unconnected with the intestinal lesion. His second report I here append in extenso.

"The portion of the intestine removed at the time of the injury was found to be the colon: it having been divided about four inches from the entrance of the small intestine. The divided portions were drawn together at the place of injury and united by organized lymph, which also connected the intestines to the parietes of the abdomen where the wound was made. The passage between the divided ends of the intestine was small and crossed by a few ligamentous-like bands—but still large enough to permit the passage of semi-liquid fæces.

"Judging from the size of the intestine removed, which was diminished by being drawn out at a small opening, we had erroneously supposed, without particular examination, that it was a portion of the small intestine, and so stated in our former communication."

In the '*Medical Times and Gazette*,' vol. xviii, page 769, is the reprint of a paper by Profes. Dieffenbach, entitled, "Case of Excision of portion of Ileum and Mesentery;" again, in vol. xx. of the same periodical is a condensed republication of the same treatise. This latter I reproduce here.

"A strong man, aged fifty, had suffered for fourteen days from strangulated inguinal hernia of the right side. Several ineffectual attempts at replacement had been made. At this time, Dieffenbach saw the patient. In addition to the usual symptoms, there was reason to suspect sloughing of the protruded parts, and escape of faecal matter into the hernial sac. An incision of about three inches in length was made into the swelling; when there escaped an ichorous fluid, with faecal matter, and portions of mortified intestine. The diseased intestine was drawn outwards, and three-inches of it, which were partially mortified, softened and thickened, together with a corresponding portion of mesentery, were cut away. A small artery of the mesentery required to be tied, and the ligature was cut close to the knot. During this process, the ends of the intestine were held by assistants. The angular incision in the mesentery was first united by ligature; and then the extremities of the divided intestine, by means of separate threads, so inserted as to bring the peritoneal coats alone into connection. The mucous membrane was not perforated. The parts were then carefully replaced. Shortly afterwards, castor oil was administered, and repeated with some croton oil, until very large evacuations were produced. These were followed by great improvement in all the symptoms. Mild aperients and the antiphlogistic regimen were the only means required during the process of cure, which was complete in the fourth week after the operation.

"The individual returned to his usual employment, which was laborious, and some weeks subsequently, after very hard work and the use of very indigestible food, he was suddenly seized with all the symptoms of intussusception, with which he died."

A careful perusal of the complete history informs us that the intestine removed had been fourteen days strangulated, during which time "fruitless attempts had been made by different surgeons to reduce it." When Prof. Dieffenbach visited the patient, he had all the symptoms which would seem to "threaten speedy death," and on the second visit "the poor man was now sinking fast, and the anxiety of death was evident." At this time the Professor operated, and removed three inches of the intestine, with a portion of the mesentery; after excision, the ends of the intestine were sutured together and returned into the abdominal cavity. Castor

oil, and croton oil, were given on the first and second days ; and though the after treatment included a course of mild purgation, this case recovered ; but, on a recurrence of obstruction from accumulation arising from his indulgence in an " immoderate meal of fat meat and other indigestible substances," he succumbed to the primary treatment, which consisted of purgatives and bleeding.

The details of operative interference of neither Professor Dieffenbach, nor Dr. Brigham, satisfy me with regard to the mode in which enterotomy should be performed, so as to secure a greater probability of the sufferer's recovery. It would have been better in both cases to have followed Nelaton's usual practice of securing an external opening, by connecting the gut to the incision in the abdominal wall ; thus the patient would be spared the risk, during " after treatment " and the surgeon much trouble and anxiety. To my mind Dr. Brigham managed the after treatment of his case " *secundum artem* " if we judge it from the standard laid down by Sydenham and Brinton ; whilst Professor Dieffenbach's patient recovered, despite a method of treatment that few would countenance in latter times.

Granting the theory and practice I have advocated to be correct, the majority of these cases were injuriously treated before operation ; and in some cases the operation was undertaken ere symptoms of urgency had manifested themselves ; and the particulars recorded as present before, and acquired after, operation, inform me that the operation in many of these cases was a remedy for a remedy (primary treatment) rather than directly a remedy for the obstruction. With an expectant or opium treatment, most of the first 11 cases could have had a great probability of recovery with or without operation. With an exclusively opium treatment I would not expect indications for operation to occur before the seventh to the fourteenth day ; and this delay is rather an advantage to the patient if he has been kept well under the influence of opium, which diminishes the shock of an operation, and keeps in abeyance peristaltic action : an item to the good for the treatment after operating.

Collapse occurring at about this period (seventh to fourteenth day) I would judge to indicate probably one of those forms of obstruction caused by twist of gut, rent in mesentery, fibrous bands, adhesion, tumours, &c., and these can only be palliated by medical treatment; and must, when diagnosed as probably existing, be subjected to operative interference; though I at present believe them to be not amenable to therapeutical remedies, there being no trustworthy evidence on record of such having recovered. Yet it is not impossible that this opinion may be proved fallacious after an extended practice of the opium method has been the subject of clinical and post mortem observation. Dr. Brinton, commenting on this point in his treatise, page 100, after discussing the probability of spontaneous cure, says—

“One or two such recoveries have to all appearance occurred in my own practice. Fortunately for the patients, however, the exact details of the process remain uncertified.”

At page 705 of *‘The British Medical Journal,’* 1876, is recorded a case of volvulus, occurring in the practice of Dr. Mc Call Anderson, of Glasgow, which appears to me as though there had been in this case attempt at spontaneous cure. Another case of obstruction from bands is reported in the same page, and had been under the care of Professor Gairdner. In both of these cases it appears to me that if by some method the gut could have been kept nearly empty, pain relieved, and constipation prolonged long enough, recovery would not have been impossible. Again, on the same page a brief report is given of a case, supposed to be volvulus, having been cured by opium and scanty diet. Experimental tests on the dog have wonderfully confirmed the hopeful prognosis Dr. Brinton had of recovery in cases where volvulus, stricture, bands, &c., existed. W. Bathurst Woodman, M. D., in the *‘London Medical Record’* for May 19th, 1875, reports experiments:—

“Sales-Girons on the mode in which the circulation of Fæcal Matter is Re-established after Ligature of Intestine. *‘La Revue Medical,’* for March 22nd, 1875, contains a paper of extreme interest which, in the absence of any name, we suppose must be attributed to the editor. He states that, whilst making experiments on the way in which temperature is affected by ligaturing the intestine, he was surprised to find that most of the dogs experimented on,

after vomiting and obstruction of the bowels, and refusing their food, began gradually to recover after the fifth day; and about the tenth day they resumed their normal appearance and all the functions of life seemed carried on as before. He thought at first that the ligature was not properly tried. This led him to make fresh experiments—with a similar result. He thinks the mechanism of recovery deserves special notice. When a segment of intestine is ligatured with silver wire, so as completely to obstruct its calibre, if the animal survive and be killed after ten days, it will be found the portion of intestine is adherent to the abdominal wall, and to adjacent coils of intestine, by false membranes, which are easily torn, and often circumscribe little collections of pus. If, without disturbing the relations of the parts, sections are made above and below the portion ligatured, we can easily demonstrate, by injecting water, that the bowel is once more pervious. There is no perforation of the bowel, but a circular cicatrix shows where the ligature was applied—the two surfaces of intestine brought into contact by the ligature are, so to speak, welded or soldered together. The calibre is normal except that at the level of the cicatrix, there is a slight circular constriction of the mucous membrane. The metal ligature is found attached to one side, and floating loop like in the calibre of the bowel. It is easy to understand that, under the influence of the peristaltic action of the bowels, the intestine is cut through by the ligature; but during cicatrization of the external coats, the mucous membranes have succeeded in separating, thus the patency of the calibre is re-established. Two things help to bring about this result. The first is the thickness of the walls of a dog's intestines: and the other, the nature of the mucous membrane preventing the adhesion of two mucous surfaces. This is then a true recovery. The cicatrix is a genuine cicatrix made up of the three tunics; and is demonstrated to be so by microscopic examination. Similar results were obtained with hempligatures. It was once found that one of these was burst by the intestinal juices. An India-rubber ligature was found encysted, its elasticity doubtless causing this result. If, instead of including a loop of intestine, the ligature was simply tied round the bowel, the results were similar, but never rapid, five days sufficing for the process. (These observations appear to the reporter of extreme interest, as illustrating the probable mode of recovery in some cases of intestinal obstruction in the human subject: and as explaining the circular (annular) cicatrices sometimes seen in the intestines in post-mortem examinations."

These experiments are examples of obstruction from annular bands, and are specially instructive from the fact that annular strictures are occasionally causes of gut obstruction in the human body. The *Medical Times*, for Nov. 25th. this year, p. 594, contains the report of a case, where post-mortem examination demonstrated the existence of "annular stricture caused by fibroid thickening of uncertain

origin.' The explanation I would give of this "fibroid thickening" would be, that this case was originally one of slight intussusception, and then there had occurred, that which usually occurs in slight intussusception—agglutination and partial absorption of the agglutinated parts. When a long portion of intestine has become invaginated, then of course sloughing of the free portion occurs; the agglutination only existing at the primary portion of the invagination. This case was treated by the "all sorts" method; that is, many remedies were given, and the last credited with the result.

The details of this case are, to me, very instructive, and especially confirmatory of my opinion, that the direct relief of the mere symptoms of constipation is not to be aimed at, inasmuch as early and repeated evacuations, as in this case, are of no benefit to the patient if the intestine has not been allowed time to become normal in condition and fit to resume its natural function. The post-mortem of this case, like that of mine, (case No. 7) revealed nothing beyond some defect in the treatment.

In the preceding remarks I have attempted to indicate the conditions justifying gastrotomy or enterotomy. Granting the former operation as being selected and performed successfully, the real question that presents itself is, "What may be the therapeutic treatment that should follow?" I can best illustrate my views on this important point by a consideration of the medical after treatment of hernia operations, which coincides with that of gastrotomy.*

Operation on hernia would be attended with greater success if before and after operation, constipation (rest of part) was well maintained by opium, and restricted and limited diet for a period long enough to enable the constricted, paralysed, and sometimes inflamed bowel, to recover its normal function. It is often forgotten that a patient's gut, with its muscular power in abeyance from or after over-distension, is practically impervious in the living

* I adhere to the term gastrotomy, though it does not correctly indicate the nature of the operation. A more correct term for the latter would be the name suggested by Dr. Ashurst—Laparotomy.

subject;* consequently it should be treated after operation as a genuine case of obstruction. These conditions occurring after the operation of hernia, explains why the patient may have no relief from symptoms of obstruction, and why post-mortem examinations some times give no clue to the immediate cause of non-success.

The dread of a few extra days of constipation during the after treatment of herniæ operations has been, and is, in these times, not unfrequently the sole cause of non-success after successful operation. Constipation should be prolonged as long as possible. The surgeon may be able, with opium and suitable diet, to delay the action of the bowel until the third week; to prolong the constipation beyond the third week is very difficult to attain. The constipation is a "consummation devoutly to be wished," rather than to be guarded against. As evidence of the opinions taught concerning the after treatment of herniæ operation, and especially illustrating my preceding remarks, I append paragraph 10 of Prof. George H. B. McLeod's paper, entitled "Remarks on Intestinal Obstructions," with special reference to diagnosis, published in the 'British Medical Journal,' for December 2nd

*Both Dr. Habershon and Abercrombie attempt to explain this condition. The latter thus expresses his observations:—"For we have seen it fatal without obstruction, and we have seen everything like obstruction entirely removed without relieving the obstruction." And the former accounts for it by the supposition that "either enteritis was present, or the bowel twisted.".... This explanation by Dr. Habershon is not consistent with his own remark, that "no cause of strangulation or obstruction was detected after death." The explanation I hold to be the correct one is this: the intestine, being confined to an area small in comparison to the length of the intestine, are necessarily packed and folded in the abdominal cavity, so that, often, they collapse at their points of replication. This in the healthy gut, is corrected by peristaltic action, whilst in a gut paralysed from over distension, correction by this means is not possible; consequently, obstruction must arise at the points of replication; to avoid which, accumulation and distension must be avoided, and rest secured, so as to regain the muscular power of the temporary palsied gut. Post-mortem examination in this condition would reveal nothing beyond over-distension, as the slightest unavoidable disturbance of the parts in examination would remove the cause: even during treatment it might be corrected by a change in the patient's position, but probably only to recur at another point. If the course of the intestines were so arranged that from the stomach to the rectum they formed nearly a straight line, this form of obstruction could not occur. Any of my readers accustomed to manipulate rubber tubing will have noticed that a sharp head of this tubing will arrest the flow of liquid, even though it is circulating when arrested, under some degree of pressure; this, however, I admit, is but an imperfect simile. No form of obstruction exists of which its cause can not be accounted for on rational basis.

of this year. Those specially interested in the question here discussed will find in this very interesting and exhaustive paper the latest and most complete resumé of the principles and treatment now in vogue, and to the refutation of which Dr. Brinton devoted much of his talent, energy, and time :—

“Paralysis of the bowel, after operation for strangulated herniæ, either by the taxis or knife, is by no means uncommon, and constitutes one important source of danger from that affection. The bowel, after being long strangulated, does not recover its function, even though restored to its place in the abdomen. Passive obstruction occurs, and unless means are used to arouse the dormant action, the patient dies.”

In the treatment of the intestine, set forth in this quotation, any attempt to rouse it would probably fail, and be the cause of prolonging the dormant state and of failure. The best method to restore action (it cannot be aroused) would be to practice an expectant method ; or, if anything is done, it should be to prolong rest, also to hinder accumulation in the gut, and its consequent over-distension with paralysis. This rule should also be our guide, even when inflammation has succeeded hernia operations. We have a parallel in inflammation and temporary distension of the bladder.

From Prof. McLeod's paper, and others read before the Edinburgh Medical Society, which can be seen in the '*Edinburgh Medical Journal*,' for 1873, 1868, 1866, it is only too evident that our medical brethren beyond the Tweed hold the principle and practice the method so general in England and Ireland ; and from what I have been able to glean, the practitioners of our art in both the old and the new continent can reform their methods of treating these difficulties with advantage.

In the treatment of these casualties, so many diverse methods and remedies are tried, and strongly advised, that I am reminded of Sydenham's remarks, “Where is the particular importance in just telling us that once, twice, or even oftener, this disease has yielded to this or that remedy ? We are overwhelmed, as it is, with an infinite abundance of vaunted medicaments.” What I hope to see and believe I shall see, general among us is Sydenham's dictum, that “By a long continuance and a frequent repetition of his (the physician's) experiments, he may lay down and prescribe

for himself a 'Methodus Medendi' from which, in the case of this or that disease, he need not deviate a single straw's breadth."

When certain symptoms are present, gastrotomy is justifiable, and gives the patient one more chance; but in my opinion converting abdominal section into enterotomy would more than treble the chance of the patient's life being saved.

This double operation—Gastro-enterotomy—was first performed for intestinal obstruction by Renault, in the year 1772, and in that case with success. In latter times M. Nelaton appears to have performed this operation for intestinal obstruction, and so frequently that it may be inferred that he had a special preference for this double operation in cases of obstruction, when operative interference was called for; and this practice appears to have been very successful with him.

The advantages attending this operation are that (supposing the gut opened above the obstruction, or should the intestine be gangrenous, this would be the site for the formation of the artificial anus, and the intestines could then be immediately and certainly relieved of their contents and symptoms of obstruction) there need not be any special anxiety as to the after therapeutical treatment, nor would there be need for a strict surveillance of the dietary. Again, if the gut invagination is adherent, as probably it would be, it should not be disturbed; or should a stricture, or any of the many pathological conditions known to occur be present, they can be ignored until nature has rectified the difficulty, which it would do in the majority of cases. Lastly, time is of no object; the gut can be allowed either one month or one year; and during that time the patient is free from suffering, and requires but ordinary medical skill and attendance.

As to external applications in these cases, it has been my habit, of late, to apply a single fold of linen steeped in cold water over the abdomen. I regret that it was not always my practice. In times past I have tortured, and, no doubt, added to the difficulties of recovery by external applications, such as linseed meal and mustard, cantharides cataplasms, hot water fomentations, linaments of supposed stimulating virtues, &c. Remedies, when

applied in contiguity to the peritoneum, or any other serous sac, no matter for what complaint, are objectionable. This is the opinion forced on my reluctant mind.*

The next class of remedies are those applied or given with the intention either of exciting or arresting the peristaltic action. To the former class belongs galvanism, the various therapeutical remedies, designated purgatives and belladonna; whilst opium and its various forms arrest peristaltic action. On the merits and correct mode of the administration of these, the patient's chance of recovery mainly depends. The frequency with which excitants of the intestines are used, even in these days, necessitates the consideration of their value. Galvanism has only been introduced in the treatment of this affection during late years; consequently our predecessors were spared this injurious torture. In those cases in which its use was resorted to during the last ten years, there has been no evidence given that it was of any benefit. It is true that in some few cases its application was followed by recovery. In those cases, its use, in my opinion, failed to thwart recovery, as the patient's powers of endurance and tenacity were more than the injurious galvanic stimulation of the intestine could wear out. Galvanism, when applied so as to effect the intestinal canal becomes a mode of purgation. And the use of purgatives, in these difficulties, I can only designate the "main force" treatment, and to me it would be quite as reasonable if immediately after a case of railway tunnel accident occurred, the authorities in charge of the "line" requisitioned the aid of an artillery corps, armed with the 'Woolwich Infant' to clear the debris; with the effect, I would

* That which first drew my attention to the subject of counter-irritation in general was, the teaching of the late Prof. Syme, that effectual counter-irritation applied on the skin over the knee joint produced irritation on the corresponding surface within the joint. Again, I noticed that an attack of Herpes on any portion of the abdominal or thoracic wall induced an obstinate localized peritonitis or Pleuritis corresponding to the track of Herpes. Most physicians must have noticed, the obstinacy of the after-pain often remains after the disappearance of the Herpetic eruption, while an attack of the Herpes on the neck, arm, or lower limb, leaves no such after pain. This latter fact indicates to me that the afterpain remaining after Herpes of the abdominal or thoracic region cannot be referred to nerve lesion, or it would follow Herpes of the head, neck, arm, or lower limb.

expect, of injuring the tunnel more than effecting a clearance of a passage for traffic. Neither argument nor records of experiences give any ground of justification for the use of purgatives at any period during the treatment of these lesions. The records of cases treated during this century unmistakably show, first, the high mortality attending intestinal obstructions; second, that persistent purgation is the principal cause of this extreme fatality. And purgation appears generally to have been persisted in upon no other grounds than that of "symptomatic treatment,"* i.e., inasmuch as constipation was present, a purgative was supposed to be indicated. As a practical illustration of the evil of prescribing purgatives, I subjoin a case of intestinal obstruction occurring in my own practice.

During the latter part of 1875, a Shetland sailor called, requesting me to prescribe an aperient to relieve some discomfort he felt in his bowels. He had already a black draught from a druggist. I neglected to examine him, and wrote a prescription for a draught of Deco; Aloes, Co. This was in the morning. In the evening of same day, I was sent for to Duncan Street, and found my morning visitor in great pain—vomiting frequently, abdomen tender and distended, indicating obstruction. I at once commenced the opium treatment, and was able to keep in abeyance pain, partially arrest vomiting, and prolong the constipation to the fourth week, when the intestines resumed their natural functions; and in the sixth week he went to sea. Had I repeated the purgatives once or twice after my first careless prescribing, this man would, in all probability, have succumbed to the treatment before the end of the first week. This opinion is based on the serious symptoms rapidly developed by the purgatives. His case was, as far as I was able to diagnose it, one of enteritis. The prescribing of an aperient, however mild, at the commencement, may do so much harm that the practitioner may fail subsequently to undo it; and as a general diagnosis is so easy at an early period,

* Dr Brinton, *Lancet*, April 11th, 1863, refers to the purgative method as "grossest malpraxis."

there is no explanation for this mistake except omission of duty on the part of the consulted, as in this case.

In the narcotic class of therapeutical remedies, three only—belladonna, tobacco, and opium,—merit notice. The two first mentioned are nearly similar in their therapeutical action on the intestines, but as tobacco is seldom prescribed, belladonna only will be considered, especially as Dr. Brinton has recorded one case in which it appeared to him to be beneficial. It is with extreme reluctance that I again venture to differ with so able and pains-taking an observer as to the value of belladonna in these lesions. My conclusions with regard to the actions of both belladonna and opium, either separately or combined, I have arrived at after experiments on the horse, rabbit, and man; and though they are not quite satisfactory even to myself, have nevertheless, given me sufficient basis for guidance to their therapeutical value. And where I have applied this knowledge in clinical practice, the results have confirmed many of the deductions already gained from experiments. The actions here attributed to belladonna and opium are sometimes at variance with what has been assigned to them by Dr. Harley, whose careful and extensive record of experiments, published in his volume on the Vegetable Neurotics,* I perused some years ago; but at that time failed to coincide with some of his deductions. When belladonna is given in these lesions, it is usual to justify the practice by asserting that it may relax muscular spasm. Disbelieving, as I do, in the existence of spasms in any conditions that give rise to obstruction, and believing further that if spasm existed, belladonna would intensify it rather than relax it, inasmuch as this drug is a stimulant to the muscular coat of the intestinal tract and to the whole sympathetic system; in fact the most valuable and effective nerve stimulant we possess in the Pharmacopia (given in medical doses subcutaneously). This action, Dr. Harley's experiments conclusively convinced me of when I first perused his volume.

* This volume instructed me on the value of belladonna as a potent stimulant, and those who may be interested in the actions of remedies daily prescribed will find in Dr. Harley's volume very valuable information.

The action of belladonna, when applied to attain dilatation of the pupil, is in my opinion, to stimulate the sympathetic ; and as the radiating muscle of the iris is supposed to be specially under the control of the sympathetic, it calls into action the radiating muscle more than the circular one, and consequently we have dilatation of the pupils. In the same way, I believe, belladonna has a stimulating action on the muscular fibres of the intestinal tract, and especially so on the longitudinal ones, under the control of the sympathetic, while the circular ones are supposed to be more under the control of the spinal system, and extra peristalsis excited by its use ; consequently this drug in gut obstruction would increase the frequency of vomiting, inasmuch as the stimulation would be developed in that portion only of the intestinal tract that is free from lesion ; the excitement of peristalsis in which would thwart any efficient rest (so much needed) at the situation of casualty where the natural function is suspended, and cannot possibly respond to any form of stimulation.*

It may be asked what explanation can be given of the recorded observations of Dr. Brinton of its undoubted efficacy in relieving pain in one of his reference cases, page 110 of his volume on "Intestinal Obstructions." This case is also more fully reported in the '*Lancet*' for April 11th, 1863, from which report it is very apparent that Dr. Brinton had no definite knowledge as to what the action of belladonna—physiologically or therapeutically is, as he refers to tobacco and belladonna, and their action on the "muscular tissues, which they cause to relax." Afterwards he expresses his belief that belladonna neutralized the constipative effect of opium ; consequently, I hold, it must excite peristaltic action of the gut, and not relax it, which would cause

* Bretonneau, Trousseau, and Niemyer refer to belladonna, and strengthen my view of its action. The latter, page 555, vol. i. says :—"Unfortunately I cannot at present distinguish the case of habitual constipation when belladonna is indicated from those in which it is not. It is to be hoped that future observations will determine the cases proper for the use of this remedy, which is so excellent in some forms of constipation." However, to discuss its value in the various forms of so-called habitual constipation would be foreign to the subject at present under consideration.

obstruction, as the contents of the gut are propelled by the contraction of the two arrangements of muscular fibres, not by relaxation.

The explanation of the supposed efficacy of belladonna in Dr. Brinton's reported cases is very apparent to myself; viz., that it had no effect at all, for he never gave the patient enough to develop any therapeutical effect; as on reference to the '*Lancet*' report I find that at the commencement of treatment a very inadequate dose of opium was given, but at a later period a more liberal use was made of opium, and with which was combined a fractional dose of belladonna; yet to the latter was attributed the beneficial result that followed. I feel certain that the belladonna in this case had no effect, for it was given in the proportion of 1-6th of belladonna to 2 grains of opium, or 1-3rd of belladonna to 3 grains of opium, the proportion Dr. Brinton mentions in this article. After careful consideration of Dr. Harley's experiments, and in particular of the crucial and careful observations of one who was always so exact in his experiments, the late Professor Bennett, as recorded in his report on the Antagonism of Medicines, and of my own experimental and clinical observations, I am forced to the conclusion, that the administration of belladonna is contra indicated in any of the varieties of lesions considered in this paper. When referring to belladonna as a stimulant of the intestinal tract, I refer to the healthy portion, as its function must be in abeyance at the place of obstruction. It has been the custom with many to administer a combination of opium and belladonna. These two remedies are, in my mind, in some degree antagonistic. From Professor Bennett's report it is very apparent that a toxic dose of belladonna will partially neutralize a corresponding dose of opium. I can better explain my meaning by supposing belladonna to be represented by S^{st} (sympathetic stimulant), and opium by S^{sed} , CS^{sed} (cerebro-spinal and sympathetic sedative), thus a toxic dose of S^{st} , i.e., belladonna, will only neutralize that portion of a toxic dose of opium represented by S^{sed} , and is wholly inoperative as an antagonist to that portion of opium represented by CS^{sed} ; consequently had belladonna been a stimulant of the cerebro-

spinal system as well as the sympathetic, it would in proportionate combination exactly neutralize the effect of a toxic dose of opium. I hold the correct antagonistic proportionate dose of atropia and morphia to be about 1 of atropia to 5 of morphia.*

It is often asserted, that the combining of belladonna and opium is so far beneficial that the vomiting, so often resulting from the administration of an opiate, is thus avoided. This is very probable, if opium is given by the mouth, inasmuch as such combination would be nearly neutral, if we except very large or toxic doses of opium.

The next remedy to be considered is opium. This, when correctly and opportunely administered, is as certain, safe, pleasant, and successful in its action as all the previously discussed drugs and modes are uncertain, unsafe, painful, and disastrous. As with belladonna, the physiological action of opium on the pupil of the eye is some guide to us as to its action on other muscles of the non-striped class. The pupil is usually contracted, when influenced by this drug. This I explain on the supposition of its special and paralysing effect on the sympathetic; consequently the radiating muscle of the iris under the special control of the sympathetic is paralysed in a greater degree than the circular one. Thus we have a contraction of the pupil. On this view of its physiological action, its effect on the intestinal canal would be to paralyse the longitudinal fibres more than the circular ones. When opium is given with the intention to benefit a case of intestinal lesion, the benefit that may accrue to the patient arises from its action on the normal portion only of the canal, by as much as possible suspending peristalsis; i.e., maintaining rest of the part to enable it to progress towards resolution of the local cause of obstruction.

Opium, to ensure its action appropriately to the treatment of these difficulties, must be injected under the skin, commencing with one-

* I note that in the experiments of Dr. Harley on the antagonistic effect of atropia and morphia in no one recorded experiment was the atropia in sufficient dose to fairly test its antidotal power.

sixth of a grain of morphia for an adult, and gradually increasing it until its sedative effect is produced, and not repeated at fixed periods, but given when, either from information received, or personal enquiry, the physician finds its action, previously apparent, gone, or wearing off.*

The injection should be given at gradually extended intervals after apparent resolution of the difficulty. For subcutaneous injection, I prefer Lig. Morph. ; Sulph., as it remains long unchanged if kept in stock, and is not so painful as the other combinations of morphia. Its advantages were brought to my notice by my friend Dr. Adam, of this town, who also coincides with my views of treatment of the difficulties here considered, and in conjunction with whom I have had on many occasions the pleasure and instruction consequent on a comparison of our mutual practice.

Opium, in the form of morphia, when injected under the skin, in adequate and opportunely repeated doses, in these cases, has the therapeutical effect : first, of diminishing the frequency of vomiting by arresting peristalsis, thus diminishing thirst, and also permitting of the solid contents of the gut becoming thoroughly reduced in consistence by admixture with the thus longer retained fluids. This result explains again how the opiate acts as an aperient ; for as soon as the consistency has been sufficiently reduced, it may pass through the paralysed or narrowed gut long before the gut has become of normal calibre or function. Second, it eases pain, and con-

* Through the kindness of my friend Dr. Wallace, I read as soon as published Vol. vii. of the "Cyclopædia of Medicine," by Dr. H. Von Ziemssen, the article on Constrictions, Occlusions and Displacements of the Intestines, which is by Leichtenstern, with comments by the Editor. In this monogram is to be found a very full, able, and systematic description of the etiology, diagnosis, prognosis, and treatment ; and there appears a decided progress towards the opium method, which he advocates, but also advises enemata of water only. A perusal of page 649, convinced me that the writers have no knowledge of the opium treatment in these difficulties, when practiced exclusive of all other modes of interference. The dread of prolonged constipation has induced a policy of treatment to be advised that must undo any benefit arising from the opiate ; in fact, it is very apparent that while opium is advised, Dr. H. Von Ziemssen has no knowledge of its effect or efficacy, when its use is prolonged, uninterruptedly, without interference with other remedies, therapeutical or mechanical.

sequently keeps in abeyance constitutional sympathetic irritation. Third, it makes the partial but prolonged abstinence from food more tolerable. Fourth, as it arrests peristaltic action along the whole length of intestine, there are grounds for believing that as soon as the contents of the gut above the obstruction have been reduced to a consistence fit to pass through the obstructed point, then the contents below are in many cases subjected to the same process of mechanical reduction of consistency. This explains to me why under the opium treatment the patient so frequently parts per rectum with pultaceous fæces. On the other hand, given by the mouth it may at times induce sleeplessness, with even delirium, or persistent and prolonged vomiting. I have observed, on many occasions, that a dose of morphia solution administered by the mouth induced obstinate vomiting, yet a corresponding dose given under the skin of the identically same solution arrested the vomit in the course of a few minutes. We know there are other remedies, some of which are either more uniform or more potent given under the skin, while others differ but slightly, either in character or effect, or potency whether introduced into the system by skin, mouth, or rectum. The subcutaneous administration of opium, belladonna, digitalis, are more uniform and potent than when given by the mouth. I can testify from experience, that $\frac{3}{16}$ th of a grain of morphia under the skin will equal $\frac{1}{4}$ th of a grain by the mouth; $\frac{1}{24}$ th of a grain of atropine by the skin, is equal to $\frac{1}{8}$ th of a grain by the mouth; 10 drops of digitalis tincture by the skin equals a dram by the mouth.

The recent investigations of Mr. Tuson, on the change of form that remedies may undergo in the alimentary canal justify me in supposing that in the administration of many of our remedies by the mouth, changes of combination and form take place; consequently physiological and therapeutical effects occur which the physician can not foresee. The discovery, lately, of the form of opium termed apomorphia, and its strong emetic action, may explain the occurrence of vomiting after opium is given by the mouth. Again, other remedies appear uniform and efficient in their action whether

given by the mouth or skin; as choral, strychnine, potass—bromide, &c.

The next consideration is the diet and drink of the patient.* The diet should be limited in quantity, and restricted as to quality, so as to prevent the introduction of an avoidable amount of excreta. But here the question arises whether the practice of this limited and restricted diet does not involve a serious amount of emaciation. In the first place it must be admitted that an avoidable degree of abstinence will involve some measure of emaciation; but it must be endured to secure the benefit arising from less excreta. A still more potent cause of emaciation is pain. It would be a much easier task to emaciate a person by induced pain, which usually is accompanied by a loathing of food, than by a minimum of diet. It is wonderful how long a sufferer can tolerate even total abstinence of food when opium is given, which appears to allay the feeling of hunger. At the period of resolution, the opium not being given so frequently, ceases to keep the appetite in abeyance. I have known the case of a sufferer from acute enteritis who retained nothing but a drink of iced water frequently repeated in small quantities for 49 days, and yet recovered at the end of that long period of abstinence not much emaciated, though previous to this complication the patient had suffered from another painful affection.

Of course it is not advisable, nor seldom necessary, to prescribe total abstinence. Art supplies us with prepared food, the administration of which adds but slightly to the difficulties which the physician has to contend with. I usually restrict the patient to the following articles of diet—arrow-root, sago, linseed tea, extract of beef filtered decoction of peas, rice water; all these being pre-

* Dr. Habershon in page 475 of his volume on Diseases of the Abdomen, while insisting on the beneficial value of opium, restricted diet and so-called stimulants—brandy and wine, &c.; yet insists also on the equally beneficial value of medicated enemata. "In this way warm water, soap, castor oil or turpentine." This reminds me of certain theologians who whilst they stoutly deny the canonicity of a certain book generally do battle the more earnestly for the remainder. The evidence contained in Dr. Habershon's reported case, shew that whatever reliance he places on opium in these affections, the enemata simple or medicated, which he recommends, more than negatives the benefit to be derived from the use of opium.

pared with water—milk being avoided. These should be administered in half ounce doses every one or two hours. The drink should be confined to iced water, given in ounce doses every two or three hours; and as the patient takes but a minimum of nourishment, it is well to economise carefully the natural heat by applying a warm bottle to the feet, and a blanket across the chest, whilst the abdomen should be exposed if practicable.

However valuable opium may be in intestinal casualties, a restricted diet is (if food can be taken) of equal importance. It has been during the last thirty years,—despite the demonstrations of physiological chemists as to the place and value of so-called stimulants,—the prevailing fashion in nearly all diseases to urge the use of what are mis-termed stimulants, as brandy, wine, ammonia, &c.; the belief in their stimulating properties being based upon no better premises than the fact of the one being pungent to drink, and the other volatile and an irritant to the nostril. With regard to food, the sufferer is frequently pressed to secure quantity as well as quality, as though the former—not digestion or assimilation—were all important. It is often forgotten that the patient is suffering from disease, distinct from abstinence; that which has to be considered is, what amount can the patient's chylopoetic viscera prepare and assimilate? else the sufferer may actually go through a process of starvation while taking a quantity that would benefit him had he been in health.

A copy of the 'British Medical Journal,' of this year, supplies us with an example of dietary which I think is a great advance in forms of diet, frequently reported as allowed to patients suffering under intestinal difficulties; yet, great as is the improvement here advised, it is capable of still further reform.

The example I referred to is contained in a lecture delivered by a court physician when discussing Enteric Fever.

"In a case, now at the fourteenth day, there is looseness of the bowels. On examining the stool, I find a separate undigested curd of milk. This curd has

acted as an irritant and induced the diarrhoea, therefore you must thin the milk, and replace it more or less by beef-tea. It has been too much the fashion to give much milk without due regard to its digestion. As remedies, you may give some starch with bismuth in enema." At the next visit, some hæmorrhage (of which the patient was kept in ignorance) was reported by the nurse. On inspection, it was found to be about half a pint of dark fluid blood. "Now, the most important point is, that this patient does not sit up for any purpose. A case which occurred during my student days impressed me very much. He had had hæmorrhage like this, but did not seem very bad; his pulse was 84; his mind clear; he was allowed to rise to the night-stool; the hæmorrhage recurred, and ended fatally in a few minutes. A mesenteric artery had been opened. You must then, by position, take off the weight of the blood-column. Omit milk altogether, the curd might irritate; give beef-tea and arrowroot; a little softened bread; a little brandy, two drachms every three or four hours, to improve the nerve-tone; give him three grains of acetate of lead with acetic acid every four hours, and an opiate enema night and morning. Observe there is no great distension of abdomen, and there is no tremor. I conclude the ulceration is not deep. *When tremor is disproportionate to other nerve-symptoms, it indicates more depth of ulceration.*" The patient did well.

• This restriction in the quality of food is a great improvement on the diet too frequently allowed patients suffering from the intestinal lesion which accompanies enteric fever, and though the lesion here is a symptom of blood poisoning, yet as soon as it manifests itself, it should be treated as though it were originally one of intestinal inflammation, with this difference, that the beneficial effect of the opium is developed by the administration of much smaller doses. Regarding the defects of the diet advised in this lecture; viz., the softened bread, and thinned milk, they appear to me objectionable, and there is no occasion to use them so long as we have suitable varieties of food supplied to us by art that are beyond question. Again, the remedies here prescribed for this form of intestinal difficulty are open to some doubt as to their correctness, as for instance the "bismuth in enema," "opiate enema night and morning." Why this tickling of the rectum after the invention of Dr. Wood's subcutaneous syringe? and I should judge that the hemostatic value of 3 grains of acetate of lead and acetic acid on 24 feet of gut would quite

equal the tonic* effect of brandy on the nerves.

The dietary and therapeutics insisted on by Sydenham, in his details of treatment for enteric fever, has not yet, in my opinion, been surpassed by any modern practitioner; his diet was more restricted as regards varieties, inasmuch as in his time, art had not supplied many valuable forms of food now so general among us.

As evidence of the prevailing mode of dieting patients after important operations in the abdomen, an instructive case occurs in the '*Lancet*' of 1876, where is reported a case of abdominal section.† In this case, all the operative details being excellently well managed, with, in my opinion, one exception, viz.; the diet and therapeutics of the intestines; the treatment of which was of primary importance to success, yet consisted of milk, "milk gruel," or "well-boiled oatmeal gruel and sugar;" third day, tea and toast; seventh day, meat and vegetables and pudding; the fifteenth day, a dose of castor oil, which was given "in order to satisfy us that the intestine was permeable."‡ And again, in the '*Medical Times and Gazette*,' of the same week, a case of Intestinal Obstruction is reported, where milk was given freely from the beginning; and about the tenth day, bread and milk; which

* During the period I had the benefit and pleasure of attending the clinics of the late Professor Bennett, he expressed his disbelief in the existence of any therapeutical tonic. To this scepticism I incline also; food, well digested, and assimilated, appears to me to be the only undoubted tonic; substitutes for the food are continually sought for even now (as the elixir vitæ in times past). It is not long ago since a learned Professor announced the discovery of a small bulk of vegetable preparation, the use of which permitted prolonged abstinence from food, with no loss of muscular power (force without fuel) even during active exercise. This discovery, I predict, will soon be numbered among the collections of unsolved riddles, such as the elixir vitæ, transmutation of metals, perpetual motion, &c.

† This case was auspicious of a new speciality—a Temperance Hospital; from which it may be gathered that intemperance in our national drink is an evil, while intemperance in food and purgative is not; for certainly the diet and purgative placed the patient in as great danger, if not more, than an over dose of brandy would have done.

‡ The late Professor Bennett, of Edinburgh, used to ironically explain the cause of the indiscriminating practice of purgation to arise "from a desire to have a clear field for future operations," which is certainly as reasonable an explanation of the cause as Sydenham's excuse, to remove, "peccant humour."

forcibly reminded me of the apple dumplings allowed in the case reported in the '*Medical Physical Journal*,' of 1824.

The reader will no doubt have noticed that some of the views set forth in this paper are speculative; and though appearing to me as highly probable, yet further clinical observation may disprove some of them. But I hold that the method of treatment—a solely, undeviating, and prolonged course of opium—herein advocated, which reason and experience indicate as the correct course, will take its place as the standard in times to come. I do not claim any originality, as Dr. Sydenham has laid down the best method of treatment up to this date, and surmised the explanation of several symptoms occurring in others. These speculations, afterwards, were demonstrated by Dr. Brinton; indeed, the latter is short of nothing as regards the etiology, but his treatment is not consistent with his own discoveries. He has, however, placed us in a position to rationalise and treat with confidence these casualities, so that they no longer “entail whole troops of remedies throughout all the stages of the ailment—remedies for the remedy rather than for the disease itself.” And though I have advocated a more persevering, undeviating, and prolonged course of opium than Dr. Brinton did, yet I firmly believe that had he lived to this time, he would have counselled my apparently extreme advice, and he would have added to the facts he has given us.

There are comments in his volume (for instance the foot-note in page 120) which show that in the rationale of treatment he had not that complete confidence in his own method which it merited.* Again, the valuable addition to our armamentarium—Dr. Wood's subcutaneous syringe—had not come into general use in his time. Consequently Dr. Brinton might not have always secured the

* See Summary, page 122, Intestinal Obstruction, by Dr. Brinton, where details inconsistent with his theoretical teachings are advised. Again, the cases given at the end of the volume as illustrations of the practise deduced from the demonstrations of Dr. Brinton; they represent an expectant method rather than an opium treatment, as the opium was given neither in dose sufficient, nor form that can be trustworthy.

intended effect of his remedies. We have no record that he ever used it.

I have appended but a few illustrative cases—they are those alone of which I have notes.

Case No. 1.—During the early part of this year, I was called to assist in the treatment of a case of supposed intussusception. The gentleman in charge of the case informed me that a fortnight previously the patient had, whilst at work, had a sudden action of the bowels followed soon after by a good deal of colic pain, to relieve which the medical attendant was called in; and he, attributing the cause to constipation, administered purgatives, enemas, &c., which however, had given the patient no relief. When I examined him I found the abdomen very tender on pressure, especially in the right hypogastric region, with moderate distension, and frequent vomiting, which had become slightly stercoraceous. I advised the discontinuance of all purgatives and all interference by the use of enemas, and ordered cold cloths to the bowels, elevation of the pelvis, and morphia administered subcutaneously night and morning. This treatment had the effect of diminishing the pain and partially arresting the vomiting, which now only occurred with a notable regularity about once in twelve hours. The distension continuing much as before, the morphia was continued, sometimes twice, and at others three times a day, subcutaneously for the seven days succeeding my first consultation, with the effect of greatly relieving the patient from pain and the partial arrest of the vomiting; but there was no diminution of the distension, nor was the rapidity of the pulse much diminished. About the eleventh day after my introduction to the case the morphia dose had to be much increased, but did not completely ease the pain, and diminish the rapidity of pulse, whilst the distension was slightly increased. On the 12th day of my co-operation in the treatment the patient's condition appeared very precarious, and indicated to my mind that if in the course of a few hours the symptoms did not improve, it would be necessary to practise some operative interference, lest perchance this might not be a case of intussusception or enteritis, but rather, one of the various forms not usually judged amenable to therapeutical remedies. This opinion was based on the fact that the morphia appeared not to have sufficient control over the pain, or the pulse. However, to my delight, when we met in consultation the next day, I was informed that the patient had passed frequent and copious semi-liquid stools, with great relief to all the symptoms. But he was still continued under diminished doses of morphia, and treated for a time as though the obstruction still existed, and ultimately recovered.

To myself, the question at what period or during what symptoms operative interference should be had recourse to, has

always been a very difficult one to decide. At the present time, I would not expect and do not think that it would be indicated (under opium treatment exclusively) earlier than about the seventh to the twelfth day; when, should symptoms of urgency appear, Gastro-enterotomy should be performed. At my last consultations in this case, I thought that very probably it was neither enteritis nor intussusception; but twelve hours delay to await the symptoms, which I judge sufficient for considering the necessity for operation, corrected my diagnosis, and probably saved the patient's life.

Case No. 2.—On the 7th of June, 1874, I was called to attend Mr. H— H—, 55 years of age. I found him suffering from slight tenderness of the bowels, with thirst, and an irritable pulse, distended abdomen, &c., and as he had been under my care for three previous attacks of enteritis, I concluded that in all probability this was a recurrence of the old complaint. I administered a quarter of a grain of morphia subcutaneously night and morning during the first four days, but the distension increased, and he vomited from once to twice daily during the first fourteen days. When the distension became extreme, I then trocared the abdomen on the most prominent point in the right hypogastric region. This operation was repeated every other day for six occasions, with the effect at the time of releasing a great quantity of gas, and totally collapsing the abdomen. I also raised the pelvis, and during the whole time of treatment restricted the patient's diet. A motion of the bowels took place on the 29th day. They continued acting for three days. Yet I continued the treatment for some time after. In the course of a fortnight after which the patient was so far recovered that he went to attend to his business, contrary to my advice. He continued apparently well, though very feeble, up to the 19th of September, when his previous symptoms returned. The morphia treatment was resumed and continued until the 2nd of November. During this period, the bowels rapidly became distended, and the distension, which was always caused by gas, was relieved by trocaring, which was performed on some occasions as often as four and five times a day, in all the trocar was inserted on 40 occasions. Relief to the bowels took place on the 22nd day of treatment, but with no subsequent action, and on the 2nd of November the patient succumbed.

This gentleman had been successfully treated for four previous attacks:—two in 1871, one in February 1873 and one in 1874; in all five attacks, which includes the last fatal one. I was unable to decide the immediate cause of death, as I failed to gain

consent for a post-mortem examination. The treatment was the same in all the attacks.

Case No. 3.—On the 10th of June, 1876, I was called to attend W— H—, 49 years of age. I found him suffering from tenderness over the abdomen, with slight distension; accompanied by vomiting, a furred tongue, thirst, and an irritable pulse. His statement was that he had been taken ill the previous afternoon with intense pain in the abdomen, and as the pain increased and the vomiting continued I had been sent for. I at once administered a dose of morphia subcutaneously; applied cold to the abdomen, and restricted the patient's diet, ordering him cold drink, limited in quantity. On visiting him the next day, 11th June, the vomiting was not so frequent, but the other symptoms continued though slightly abated. I repeated the injections of morphia. On the 12th June, I found the general symptoms had much improved. I continued the injections. On the 13th, I found the symptoms continuing to improve and the distension and tenderness very perceptibly diminished. On the 14th, the vomiting had ceased, and the patient suffered but slight thirst. On the 15th, all the primary symptoms were absent, and I then prescribed a mixture of tincture of opium, a drachm in half a pint bottle of water, and ordered a table spoonful to be taken every three hours. This he continued to take during the subsequent 8 days. On the 23rd of June, the bowels acted copiously. After this the opium was continued for a week, his diet restricted, when his recovery was completed.

This I judged to be a case of enteritis; probably, total abstinence from food, and limiting the patient to cold water to drink, and the omission of medicine, would have been sufficient in this case, with strict confinement to the horizontal position while in bed.

Case, No. 4.—On the 31st of May, 1872, I was called to attend W— G—, aged 40 years. This man had fallen from the mast head of a ship. I visited him in the evening of the same day and found him suffering from a severe injury of the spine, which had paralysed the lower extremities; and causing also retention of urine, which had to be removed twice a day for several subsequent weeks. On the 3rd of June, the patient began to be disturbed by frequent attacks of vomiting, the bowels became tender and distended, and the tongue dry. I judged these symptoms to arise from traumatic enteritis, and I commenced at the outset to administer morphia subcutaneously night and morning. The distension in this case gradually increased up to the 27th day after the accident, when spontaneous action of the bowels took place; and was followed by a slight diarrhoea, lasting some days, though the opium was continued.

This patient recovered perfectly as regards the enteric complaint, but was two years before he began to regain power in the paralysed parts. The distension in this case was enormous, yet not at that time possessing sufficient confidence in the practise of trocaring the abdomen under such a condition I did not venture to practise it. This case reminds me of another one of traumatic enteritis, in which there was enormous distension, which I saw about 14 years ago. I was called to attend a railway porter, a stout middle aged man, who had been crushed between the buffers of two railway carriages, the force being applied to the abdomen, to the right, just above the umbilicus, smashing his watch, which was in his waistcoat pocket. The treatment I practised was opium, restricted diet, &c., and so averted the threatened symptom of traumatic enteritis; and in the course of two weeks he was apparently well, and declined any further treatment. I cautioned him, but in vain, he commenced to feed himself with the usual routine articles of diet, and not being pleased with my previous warning, and thinking my restrictions an unnecessary curtailment of his liberty, he requested me not to visit him again; however, I was again sent for, but not until intestinal obstruction, with abdominal distension, had re-occurred, with sloughing of the abdominal wall corresponding to the position of the watch crushed, where also I noticed the intestine protruding and sloughed, and discharging its contents externally. This case was fatal through the want of ten or fourteen days further restriction from food, and in all probability the inflammation involved some portion of the colon, and the accumulation occurred in the cecum with hyperdistension and gangrene of a portion of the colon. Had the surgeon in charge of the case during the second attack used the trocar, this mishap might have been avoided.

Case No. 5.—On the 11th of March, 1876, I was called to J—W—, 25 years of age, residing at Liverpool.—I found that the patient had been rather ill on the 7th of March, with an attack of what he and his friends thought was simply a bilious attack. For the three subsequent days they administered purgatives, which had only aggravated his symptoms. When I visited him I found slight distension and tenderness, and constant vomiting. I administered a dose of morphia subcutaneously, which had the effect of diminishing the vomiting, pain,

and distension ; and I ordered a mixture of tincture of opium, one drachm, in half a pint of water, a table-spoonful to be taken every two hours. On visiting him the third day the symptoms were so far improved that the patient continued the mixture in diminished quantity, and rapidly recovered, having a spontaneous action of the bowels on the tenth day. ●

This I thought was a mild case of enteritis.

Case No. 6.—On the 15th of December, 1874, I was called to attend a club patient of mine, Mr. P—M—, 32 years of age, residing at R—Street. I found him suffering from intense pain in the abdomen, attended with constant vomiting, which was stercoraceous in character. He had been sick some days, but judging that it arose from simple constipation he had used purgatives ; consequently, I found the symptoms much aggravated when called to attend him. I immediately injected a quarter of a grain of morphia beneath his skin, and continued to do so twice daily, for three days, with the effect of diminishing the pain and decreasing the vomiting ; but the distension increased. On the 4th day, I commenced to inject under the skin a $\frac{1}{4}$ of a grain of morphia four times a day, and on the 6th day finding the distension still increased, he was tapped with an ordinary bladder trocar with the effect of relieving him of a good deal of gas which collapsed the abdomen. On the ninth day the trocaring was repeated, the morphia being still continued. The use of the trocar was repeated, at intervals of three days, and on four occasions in all. On the twenty first day, a spontaneous action of the bowels took place, the patient being much relieved ; yet the opium treatment was continued for some days, and the symptoms gradually resolved, and the patient recovered.

During the treatment of this case, (the patient being an occupant of one of those filthy dens with which Liverpool abounds,) necessity compelled us to be satisfied with what nourishment was contained in a little cold water, in which a portion of Leibeigs extract of meat had been dissolved. This was the only nourishment the patient had during the whole time, and he consumed four one ounce jars of this commodity ; and as an example of the knowledge of dietetics possessed by this man's partner, no sooner were the symptoms relieved than she prepared him a cabbage as a delicacy.

This is instructive as showing how well the patient progressed with all the disadvantages arising from primary purgation, filth, bad nurse attendance, vitiated atmosphere, and the absence of

anything approaching comfort, beyond a hard bed, and a shed over head; but he had two great advantages, a very slight amount of nutriment was administered to complicate his case, and free administration of opium, which also assuaged his sense of hunger. These two latter assisted in bringing about resolution, which occurred at least one week prior to my expectation.

Case No. 7.—Mr. C. of D—Street, consulted me on the 1st of August last, suffering from an attack of diarrhoea. I prescribed tincture of opium, muriate of ammonia, and chloric ether, and he got apparently better in the course of 7 or 8 days, though still confined to bed. On the 10th of August I was sent for and found him suffering from severe pain in the abdomen, accompanied by vomiting, thirst, with tongue dry and furred, pulse 9 in 5 seconds. I injected a quarter of a grain of morphia under the skin night and morning. Advised cold drinks, beef tea, arrowroot and sago, made with water and linseed tea; his diet to be confined to these in an ounce dose, given every one or two hours, and cold water as a drink given frequently, but in small quantities. On the second day I found less tenderness of the abdomen, but more distension, diminished thirst, vomited once of a bilious character, pulse eight in five seconds, continued the morphia, subcutaneous injections night and morning. On the third and fourth days, more distension and slight thirst, pulse seven in five seconds, vomited once in thirty-six hours, not much pain. On the fifth day, vomited twice, pulse eight in five seconds, increase of thirst, tongue slightly furred, little more tenderness, distension not increased; the dose of morphia was increased to half a grain under the skin three times a day. Sixth day, vomited only once in twenty-four hours; pulse, seven in five seconds; less thirst; tongue clean and moist; no pain; distension not any less. This condition remained unexchanged during the seventh, eighth, ninth, and tenth days of treatment. On the seventh day, the lower legs of the bed were raised a foot to elevate the pelvis, ease distension, and facilitate vomiting. On the eleventh day, vomited twice; the vomit being stercoraceous in character. Half a grain of morphia was given under the skin four times during this day; the pulse on this occasion being eight in five seconds, and slight increase of thirst. On the twelfth day, he had diminished thirst, and had only vomited once in forty hours, the vomit was stercoraceous in character. Morphia was given under the skin three times this day. The condition and treatment continued the same on the three following days. On the sixteenth day, though the distension was not so extreme as to make the use of the trocar urgently necessary, he was trocared with a No 3 size trocar; a small volume of *flatus* escaped; no perceptible effect upon the abdominal distension; continued the morphia as usual, the vomiting being absent at this time for thirty-six hours. On the eighteenth day, vomited

once, moist tongue, pulse six in five seconds, temperature ninety-nine and half. On the twentieth day, accompanied by my friend Mr. Rushton Parker, the abdomen was trocared again; about five ounces of fluid escaped, and some gas. It had slight effect upon the distension, which was however not very extreme. On the twenty-first day, the vomit ceased to be stercoraceous, becoming rather serous in character, and occurring about once in twenty hours. During the twenty-second, twenty-third & twenty-fourth days, the pulse varied during these days from six to 7 in five seconds, temperature ninety-nine to ninety-nine and half. September 3rd, the twenty-fifth day, action of the bowels occurred, and a motion composed of about 1 lb. of pultaceous fœces was passed, distension of the abdomen perceptibly diminished; pulse seven in five seconds, tongue moist, very slight thirst, slight pain. On the twenty-sixth day there was no change, the morphia treatment was intermitted for a day. On the twenty-seventh day only half a grain of morphia administered once during the day. On the twenty-eighth day increase of the distension, with a good deal of pain, pulse became small, and nine in five seconds; administered half a grain of morphia three times a day. On the evening of this day, assisted by Mr. Rushton Parker, I trocared the abdomen, and removed a quart of stercoraceous fluid which diminished the distension, and after so doing administered a grain of morphia under skin. About two hours after, the patient vomited fluid, not stercoraceous, and passed per rectum in one hour after the last vomit fully three quarts of pultaceous stool. In three hours after this evacuation, I was summoned to see him at two a.m., on the twenty-ninth day, and found him very prostrate, pulse ten in five seconds, abdomen perfectly flaccid and hollow, no appearance characteristic of collapse, but great pain. I administered half a grain of morphia per mouth, as he expressed himself in much pain. The pain, I was afterwards informed by his attendant, continued up to 8 a.m., when he died.

My friend Mr. Rushton Parker did me the favour of making a post-mortem examination of the abdomen.

"Body spare, belly flat. Whole surface of peritoneum shining, and injected, except at contiguous margins of intestines, which adhere by the medium of a little puruloid lymph. In the belly, about a pint of thin, tumid fluid containing a little fecal matter, coils of small intestine, occupy the entire front of the abdominal interior and lie chiefly transversely. A double coil lies above and in front of the omentum, the tip of which occupies the internal inguinal aperture, whence it is easily withdrawn. The whole of the colon is collapsed; the rectum is little distended. The small intestine is everywhere slightly distended with fluid, and its walls moderately thickened. At the ileo-cæcal portion is a hard nodule, consisting of an annular thickening of the bowel about $\frac{3}{4}$ inch long, and on being slit, invagination of ileum. The layers of bowel are

"firmly united together by fibrous tissue, which completely seals over and smooths the peritoneal part of the fold, one half of the circumference. The thickened portion has a canal through it, about large enough to hold a cedar pencil, and here the mucous membrane is quite red, and covered with a little foecal matter. No trace of the vermiform appendix remains, nor, can the ileo-coecal valve, or anything representing it, be recognized. Left kidney was completely atrophied, right one much enlarged but healthy, no examination was made of the thoracic viscera."

From which it may be noticed that the part intussuscepted was patent; there was no trace of the punctures of the trocar. The prior history of this case throws some light on the real cause of death. He had been my patient for nearly twelve years; first, fractured leg, then syphilis, rheumatic fever, erysipelas of leg, ulceration of leg, and frequent ailments of short duration; and had been at one period irregular in his habit of living. All these antecedent complaints had depreciated his store of vitality, so much that he had not in store sufficient stamina to outlive the test this serious lesion involved. Dr. Brinton, in '*Lancet*,' vol. i. 1863, gives a case typical of this one where the patient's cactexia solved the cause of death. My case also was one in which the sufferer was much enervated from years of previous occasional ills. In the treatment of this case I have thought since that the omission of the morphia on the 26th day was not advisable. Again, when summoned to see him on the 29th day, at 2 a.m., it would have been better practise had I given the morphia subcutaneously, as from the evidence of the attendant I afterwards learned it had no effect by the mouth; but noticing the marked remission of all the symptoms of obstruction, this fact induced me to relax some of my vigilance, and to give the morphia by the mouth, a method I am certain is frequently useless. Another act in the treatment of this case I regret. On the 25th day after the first action of the bowels, I placed the patient horizontally, which I fear induced greater mechanical pressure at too early a period; this I judged from the symptoms which followed at the time, and consequently restored him to the inclined position on the 26th day, with marked improvement, so that I was induced to omit the morphia that day. I prognosed that this was a case of in-

tussusception;* and the post-mortem confirmed my diagnosis, which might be more strictly expressed as a surmise rather than diagnosis; and those only, in my opinion, who have had frequent opportunities of watching these cases, know the uncertainty, and are alone able to guess with any probability of success the nature of the obstruction. It appears to me that experience only can guide the practitioner to indicate which of the many causes of obstruction may exist, the symptoms attached to the varieties of causes being so much alike, and differing slightly and palpable only to the accustomed observer.

Case No. 8.—On December 16th, last year, at midnight, I was requested to go on board a Swedish vessel, in the Salthouse dock to render professional assistance to one of the crew. On boarding her, I found the steward in great pain. His previous history was, that while “straining at stool,” he felt a sudden pain in the right iliac region, no diarrhoea, thirst, slight acceleration of pulse. The captain, on the occurrence of the pain, had given him a dose of Epsom Salts. This he had vomited immediately. My examination of the patient was made in about one hour after the accession of the pain. I at once injected under skin a quarter grain dose of morphia, advised abstinence from all food, and allowed a limited quantity of drink, frequently repeated if desired by the patient. Next day, at 9 a.m., I sent my assistant to visit him, with instruction that if in pain, to inject an eighth of a grain of morphia. This dose was given, and at 4 p.m., visited him myself, and was informed that he had vomited twice (but slight in quantity) during the night. The iliac region was still tender, slight thirst, pulse accelerated. I now again injected a quarter of a grain of morphia under skin, previous details as regards diet and drink to be adhered to. Third day visited and found that he had vomited once only since last visit, all the other symptoms being the same as those present on the second day. I now advised removal to my hospital, but before removal injected $\frac{1}{4}$ grain of morphia, 2 p.m. In the evening I visited him in the hospital, and found tongue more furred, no increase of distension, tenderness still present, on pressure of the iliac region, temperature

* Though intussusception is usually followed by some degree of inflammation and adhesion at the primary portion of the invagination, yet intussusception has occurred without any adhesion at the primary portion of the invagination. This latter condition of invagination is extremely rare, and requires no exceptional treatment, even if it were possible to diagnose it. Consequently, the practice of injections of water per rectum, or inflation with air or gas would probably be a “kill or cure” experiment. A recent number of the ‘*Student’s Journal*,’ vol. v., page 44, reports a case that forcibly illustrates the danger of interference per rectum after the first few hours. In that case, hydrostatic pressure was employed, the pressure used being that of a column of water four feet high—perforation of intestine resulted.

100, pulse as before, had vomited once this evening, repeated half grain of morphia. On the fourth day, at 9 p.m., injected half grain of morphia, symptoms present during this day, pulse no change, no vomit, tongue furred, slight tenderness and distension, less than had hitherto existed, temperature 102. In the afternoon he passed suddenly a very copious liquid stool. No food was allowed until the fourth day; a little arrowroot and water and beef tea was allowed this day in response to the patient's request, and another half grain of morphia was administered under the skin at 10 p.m. Fifth day, 9 a.m., gave $\frac{1}{2}$ grain of morphia under the skin; at 12 noon, passed a copious pultaceous motion, pulse 7 in five seconds, temperature 101, tongue furred, no vomit, slight thirst, $\frac{1}{2}$ grain of morphia given at 6 p.m., not the slightest tension of the abdomen, but slight pain in right iliac region on pressure. Sixth day, had during night passed several small pultaceous motions, pulse 7 in 5 seconds, temperature 99, tongue less furred, no distension, pain in iliac region diminishing, no morphia given in the morning, patient wanted to return to Sweden, but with the assistance of two other medical friends was persuaded to remain another week; 10 p.m., $\frac{1}{2}$ grain of morphia under skin, as there was increase of pain. Seventh day, pulse and temperature normal, tongue correct, no thirst, pain only on firm pressure of iliac region, bowels acted three times during the day; $\frac{1}{2}$ grain morphia was given at 10 p.m., same diet continued. Eighth day, apparently well, but still slight pain on firm pressure, continued same diet, and $\frac{1}{2}$ grain of morphia under skin at bed. Ninth day, all abnormal symptoms absent, and the evacuations passed appeared of normal consistence, though small in bulk, indicating that all accumulated liquids had been passed, consequently the conditions permitting the spurious diarrhoea which often follows relief of obstruction existed no longer.

Never before had I been consulted so early after the accession of the symptoms of intestinal difficulty as I was on this occasion. In this case I prescribed in one hour after the commencement of the obstruction, as indicated by pain. This case was well on the sixth day. How early relief of obstruction can occur it is difficult to say. Sales-Giron's experiments show that about the fifth* day in

* The '*Medical, Physical and Surgical Journal*,' for the year 1823, contain a report of a case of invagination of the small intestine, reported by Mr. T. Bush, in which the obstruction ceased on the fourth day, and on the eighth day a piece of gut fifteen inches long was passed per rectum. Mild aperients were given in this case. Records and observation incline me to believe that while it is possible for invagination of a portion of either the small or large intestine to separate so early as the fourth day, yet intussusception involving the ilio-cæcal valve would take a longer period, and intussusception involving this part is more serious than when it occurs in any other locality. Annular stricture, though to make this artificial stricture the dogs had to be gastrotomized, had one advantage, mercifully, the benefit of expectant treatment. Those who cannot be convinced of the value of the opium method will find their success increased by an expectant one compared with the "all sorts" treatment now in vogue.

dog, resolution takes place even in one of the most serious of intestinal obstructions.

Case No. 9.—*Case of the young man at the Boys' Home.*—The patient, a young man 20 years of age, on the 8th of January last, partook of a hearty meal of hashed flesh and vegetables, at 6 p.m., but two hours after going to bed he was awakened with a great pain in the left hypogastric region, which was soon followed by vomiting, which continued during the whole of the two following days, the 9th and 10th. On the evening of the latter day a message was sent to my house, which however was not delivered to me. On the morning of the 11th a second message was sent, on the receipt of which I at once visited the patient. I found him in bed, in a stooping position, which he had assumed in order to get some relief from the pain he was suffering. On examination, I found the tongue furred and the patient suffering great thirst, with constant vomiting, accompanied by acute pain in the left hypogastric region. The abdomen was tense, with moderate distension, pulse 10 in 5 seconds. This was his condition at 10 o'clock in the morning of the third day of the attack. I immediately injected under the skin $\frac{1}{4}$ grain of morphia, which at once arrested the vomiting. I visited him again at 11 a.m., and found the same symptoms present with the exception of the vomiting, which had ceased; I injected another quarter grain of morphia. At 2 p.m. there was no relief of pain. At 11 p.m. the pain still continued, and the patient had had no sleep. I injected $\frac{1}{4}$ a grain of morphia, the pulse being nine in 5 seconds. There was no vomiting. January 12th, 10 a.m.—The patient had slept a little during the night, pulse ten in 5 seconds, thirst, and furred tongue, temperature 102, a slight diminution of pain, no vomiting. I injected half a grain of morphia and had the foot of the bed raised two feet, which latter was followed by a marked relief of pain, so much so that the patient began to sleep before the opiate could have had time to have any effect. At 5 p.m., the pulse was eight in 5 seconds, and the pain was much diminished though the abdomen was still tense. The patient expressed his gratitude for the relief he derived from the inclination of the bed. At this visit I injected a $\frac{1}{4}$ grain of morphia. At 10 p.m., I found the pulse 9 in 5 seconds, temperature 100. I tried the effect of lowering the couch, which was followed by an increase of the hypogastric pain; it was therefore restored to its previous height. Half a grain of morphia was injected at this visit. January 13th, 10 a.m.—Pulse 9 in 5 seconds, temperature 100, tongue improved, abdominal distension as before. Had slept a little during the night; 12 noon, pulse 8 in 5 seconds, no pain but slight thirst. Had not vomited since the first day of my visiting him; 11 p.m., I was accompanied by Mr. R. Parker. We found the pulse 9 in 5 seconds, temperature 101 $\frac{1}{4}$, distension moderate, pain only when disturbed. I injected one grain of morphia. January 14th.—Pulse 8 in 5 seconds, temperature 101, distension diminished, no pain,

bed had been lowered ; the patient had slept well during the night, no vomiting. 12 noon.—I injected half a grain of morphia. 10 p.m., continued the morphia. There was no pain, no vomiting, and no action of the bowels had yet taken place since the first day of the attack. Had slept a good deal during the day. Injected $\frac{1}{2}$ grain of morphia. January 15th.—Pulse 7 in 5 seconds, temperature $98\frac{1}{2}$, distension much diminished, slight thirst. Had slept well during the night ; no vomiting and no pain. Injected $\frac{1}{2}$ grain of morphia. 11 p.m., distension diminishing, no pain, pulse 7 in 5 seconds ; no vomiting. January 16th, 10 a.m.—No distension, pulse 6 in 5 seconds, no pain, no vomiting, neither action of the bowels. At 2 a.m., however, of this day, the 8th day of the attack, the patient's bowels acted, the stool being of natural consistency. At 11 p.m., pulse 6 in 5 seconds, no pain or thirst ; injected half a grain of morphia. January 17th, 10 a.m.—Pulse and temperature normal, no distension or pain ; the bowels had acted. At 8 a.m., fæces pultaceous in character. January 18th.—On this day the patient was allowed to leave his bed, with instructions to restrict his diet, and having by this day secured, as I thought, perfect recovery. January 19th.—I was sent for in the evening, the patient being reported as not feeling so well, and on arriving at the Home, was informed that the sufferer had disregarded my advice, and regaled himself at noon with a hearty meal of hot toasted bread and tea, with the effect of causing some return of the hypogastric pain. I interdicted all food, and gave him a course of subcutaneous morphia. During the 20th and 21st of January, with the result of arresting the recurring symptoms, and by a more strict surveillance of the patient by the superintendent of the Home, he recovered perfectly.

1. The conclusions I have arrived at since I have interested myself in the history, causes, and treatment of Intestinal Occlusions are, first, that we have retrograded during the period that has elapsed since Dr. Sydenham published his opinions and practice on the value of an opiate treatment.

2. That we have no published record that Dr. Brinton secured the benefit that is now possible to gain from the administration of opium.

3. That the causes which give rise to gut obstructions may be traumatic, irregular peristaltic action, straining, lead poisoning, ingesta, inflammation, prolonged drastic purgation, and mechanical derangement, as volvulus, rent in mesentery and hernia, &c.

4. That early relief of constipation, and gradual diminution of distension, indicate enteritis or peritonitis, and the more prolonged

the constipation the more fluid the evacuation when it does occur. Again the relief of constipation is not a trustworthy sign that the patient is no longer in danger.

5. From the category of obstructions I exclude a merely loaded rectum, which usually arises from neglect on the part of the patient to respond to the call of nature, and which can be at once detected by digital examination per rectum and by the parturient character of the symptomatic pain. This condition is an extremely rare complication following obstructions treated by opium and restricted diet.

6. That distension and pain are to be averted if possible, and when present diminished, if their complete removal is not possible. The symptoms of constipation are to be ignored in treatment, as the more prolonged the constipation the more fluid the evacuation when it does take place.

7. When vomiting has been absent 2 to 3 days, it is highly probable that the gut is patent, though there be no evacuation of the gut contents per rectum; as the contents below the obstruction, if not already of pultaceous consistence, are subjected to the same process as occurred to the contents above. As soon as this has been effected, the opium will no longer constipate.

8. Semi-fluid feces do pass through the abnormal part before peristaltic action has returned in the part; diminution of vomiting is to be aimed at, yet its total cessation, while the obstruction exists, often precedes collapse. The character of the vomit is not a serious symptom, if it is not too frequent.

9. Treatment may be briefly stated as a method which involves a restriction of quality and quantity of food, and limited libations of drink, and restraint of peristaltic action in the normal portion of the gut.

10. That there are only two indispensable remedies required for the treatment of these difficulties—opium and gastro-enterotomy. The former is indicated in every case, while the latter is required when symptoms show that the former must fail.

11. There should be no interference per rectum after the first few hours. The indications that should guide us in the use of opium are, the pulse, vomit, and pain. Opium, when given in efficient doses, decreases the frequency and increases the volume of the pulse, and makes the act of vomiting more seldom and vomit more copious, while pain is sometimes totally relieved without producing stupor. The treatment of typhlitis is no exception to the rules here laid down.

12. The diet should be carefully regulated for a period after, as well as previous to, the resolution of any difficulty.

13. Those therapeutical remedies which are injurious, as well as those that have a beneficial action, can act only through their effect on the healthy portion of the intestines. We possess no remedy that will affect the obstructed part, except interference by operation.

14. That trocaring a distended colon is necessary if accumulation is straining the ilio-cæcal valve.

15. When operative interference is indicated, that advised and practised by Nelaton is the most advisable, to incise the abdominal wall in the right side above Poupart's ligament, attach to the incision the first distended coil of intestine that presents itself; then puncture the gut and ignore the primary difficulty. This operation is easily performed, adds but little if any to the danger; and should it be performed on patients in progress to recovery (unnecessary operation) I cannot believe that it can add any to the patient's difficulty, but confidently I believe the reverse.

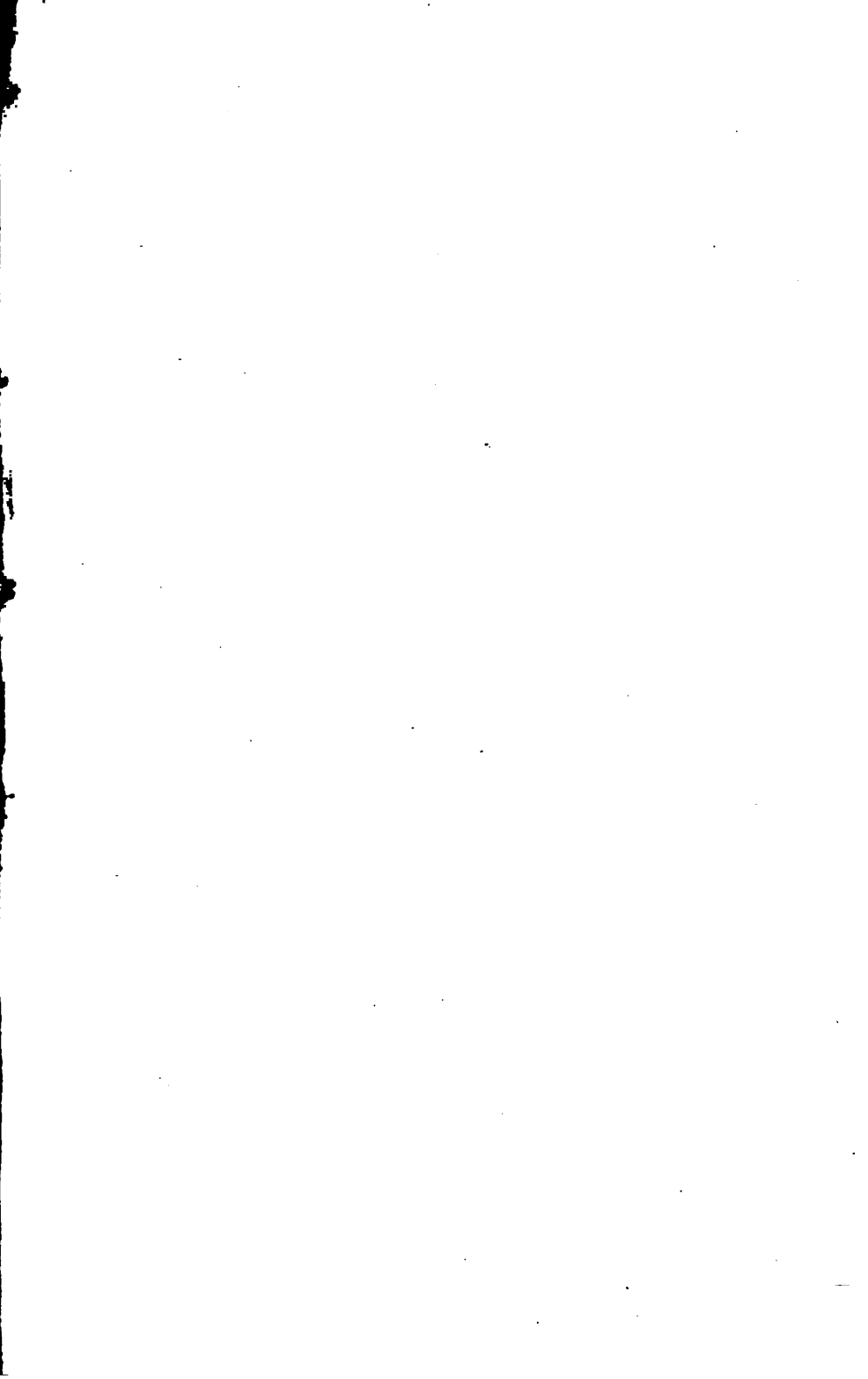


Fig. 1.

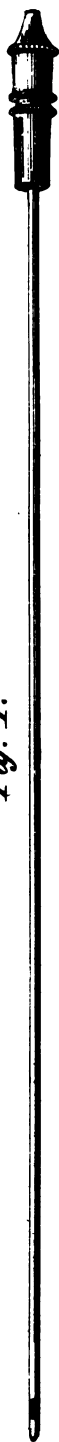


Fig. 2.



Fig. 3.



Fig. 4.



Fig. 5.



PLATE I.

Fig. 1 shows male canula to fit trocar (fig. 2.)

Fig. 3 represents canula and trocar combined.

Fig. 4 is a full-size drawing of subcutaneous syringe, which is so constructed that it can be charged with several doses of the drug, the dose is regulated by the withdrawal of the piston, thus combining a cutaneous syringe and reservoir for the medicine.

Fig. 5 represents cap to protect the needle, with ring to attach to watch chain, or as may be most convenient.

PLATE II.

Represents an aspirating pump, which I designed, and which was made for me by Mr. Reed, of Harrowby Street, Liverpool, and can, if needed, be attached with rubber tubing to canula and trocar, shown in plate 1. E indicates the cap of oiler; D the exhausting force or rubber ball; the centre opening always discharges the aspirated liquid, the suction force can be applied to either end (right or left arm.) I have used this for a long time, and can recommend it as simple, will last many years, even if in use many times daily, or if laid aside for a time, is always in order and ready for use, cannot possibly get out of order, it can also be used as an enema or stomach pump.



BACTERIA NOT ESSENTIAL TO INFECTION.

By JAMES ROSS, M.D., M.B.C.P., Manchester.

*(Read before the Meeting of the British Medical Association, at Sheffield,
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THE term infection is used here in its widest sense. It is meant to include every case in which disease is communicated, either from one to other organisms, or when it spreads from a local centre in an organism so as to affect other parts of the same individual. The term Bacteria is also used in a generic sense to include all those lowest organisms which are found in putrefactive fluids. Cohn divides them into four groups.

1. Sphaerobacteria (Kugelbacterien) corresponding pretty nearly to the Micrococci of Hallier. These exist separately, or come together to form filaments, or colonies, or form masses embedded in a gelatinous substance called by Cohn Zooglea.

2. Microbacteria (Stäbchenbacterien) of which the Bacterium termo found in putrefactive fluids may be taken as the representative.

3. Desmobacteria (Fadenbacterien) of which vibrio is an example, and

4. Spirobacteria (Schraubenbacterien) examples of which are spirillum—leptothrix buccalis—and Sarcina ventriculi.

In Wagner's handbook of General Pathology, from which I have borrowed this classification, it is asserted that the Bacteria of infective fluids belong to the first group. I am anxious to quote Wagner's authority upon this point because he is a believer in the theory of the parasitic origin of infectious diseases. I have, however, assured myself by repeated observation that the particles found in the infective fluids of a large number of diseases are similar to the Micrococci of Hallier, and when due care was taken to exclude organisms from without, I have never found the ordinary putrefactive Bacteria in these fluids. In a case of puerperal fever recently under my care, I repeatedly examined the washings from the uterus after injection of Condry's fluid, and contrary to my expectations I did not find a single Bacterium termo; but there were innumerable particles similar to the Micrococci. I will not assert that rod-bacteria are not met with in cases of Septicæmia, not having had the opportunity of observing a case of the kind lately, but judging from the descriptions of the observations of others, one of the most characteristic features of such cases is the numberless spheroidal particles which are contained in the fluids of the body.

Bacteria exist in an active and passive, or motionless condition, and Wagner asserts that the Sphærobacteria belong to the motionless class, and that the movements which they exhibit, when examined under the microscope are the well known Brownian movements. This assertion is, in my opinion, though in the main correct, somewhat too absolute. I am inclined to think that the vaccine particles which Wagner would include in his group of Sphærobacteria, have a motion peculiar to themselves, over and above that which is to be attributed to the Brownian movement. But however this may be it is quite certain that it is very difficult to distinguish between micrococci and inorganic particles, and dead particles of organic matter, and this can only be done by applying chemical tests. But for my purpose, the most important statement which Wagner makes is that it is almost impossible to distinguish micrococci from certain granules and molecules which are found in the higher organisms during health. Most of the

latter, he says, dissolve sooner than micrococci in acetic acid or caustic alkalies, but he adds that the pigmentary nuclei of the choroid resist the action of these reagents as long as the micrococci themselves. When therefore a gentleman declares that he has seen bacteria in a certain fluid, we are entitled to ask him whether he has seen the rod-bacteria whose active movements of translation there can be no possibility of mistaking for any thing else, or whether he has seen sphærobacteria, whose movements, if they have any peculiar to themselves, are all but indistinguishable from the Brownian movements. And if he says that he has seen sphærobacteria, we are entitled to ask him what tests he has applied in order to distinguish between them and other elements of the body. In the records of observations which I have read no real assurance upon this point is given, and I have the strongest possible opinion myself that what have been described as Bacteria are in reality free granules, and specks of protoplasm elements derived from the tissues of the body, and not true organisms at all.

I injected three minims of strong liquid ammonia into the thigh of a frog. Two days afterwards the thigh and leg were found much swelled. A puncture was made in the thigh, and a small quantity of sero-sanguinolent fluid withdrawn by a capillary tube. The appearances found when the fluid was examined by a 1-12 immersion are shown in figure 1. Several altered red blood corpuscles were found; half a dozen of them appearing upon the field of the microscope at once; but after a careful examination of the whole slide, only two or three full sized white blood corpuscles were noticed. The corpuscles present were generally not more than the 1-5000th of an inch in diameter, and there were all intermediate sizes down to particles not more than the 1-20,000th of an inch. The separate particles—the so-called Bacteria—were very numerous and very active, but when acetic acid was added to the fluid, the granules of the corpuscles were found to resist its action as long as the free particles; and when the protoplasm was dissolved the cell granules could not possibly be distinguished from the free particles; and indeed there is every reason to believe that in this case the free particles were simply granules derived from the

disintegration of the cells. The cells were much less in size than the white blood corpuscles, their outlines were also more irregular, and some of them were so small that they were only represented by a single granule embedded in a speck of protoplasm. And if we imagine the disintegration carried one step further, by granules becoming free without protoplasm, then we get a particle which cannot be distinguished from a micrococcus. This idea is still further strengthened by an examination of fluid taken from the margin of the inflammation of the frog's thigh, as shown in figure 2. No red blood corpuscles were met with, but under each field of the microscope about half a dozen full sized white corpuscles were seen, and although there were many of intermediate sizes between these and the free particles, yet on the whole there were fewer of these, and the number of free particles were much less abundant. It is difficult to interpret these facts except on the supposition that as the inflammatory action had been less intense at the margin, the disintegration of the cells had gone on to a less extent, hence the cells on the whole were of larger size, and there were fewer of the free granules.

The morphological elements in pus from a pyœmic abscess which I examined were very similar to those found in the fluid from the frog's thigh. The corpuscles were on an average less in size and more granular than those of healthy pus, there were a large number of small corpuscles with only two or three granules, and many with only one, and there were a large number of free particles in active movement,—the so-called Bacteria. I could not myself make any distinction whatever between the granules embedded in the protoplasm of the corpuscles and the free particles. The general effects produced upon the frog by the injection of the ammonia into its thigh are very interesting in this relation. If any general effect were produced we should expect, according to the Germ theory, that it would arise from the accumulation of Bacteria in the capillaries of various parts of the body; but on examining the circulation in the mesentery, four days after the injection, the condition found was very different. Large numbers of white blood corpuscles surrounded the blood vessels, reminding one very

forcibly of what occurs in the mesentery of guinea-pigs suffering from tuberculosis; the most marked difference between the two conditions being that the corpuscles in this case contained more granules than those in tuberculosis. I have the preparation of the mesentery still in my possession, and fig. 7 shows the condition around a small vessel. This shows the close relationship which exists between the white blood corpuscles and these so-called Bacteria, a relationship so close that it is extremely suggestive of a causative nexus between them. And if we turn to the specific fevers, we meet with more or less analogous facts. If a drop of glycerine be placed on a glass slide, and the slide be shaken near a patient during the desquamative stage of scarlet fever, a great many epithelial scales and free granules will become entangled in the glycerine. Many granules may also be seen embedded in the epithelial scales, like stones in mortar; but why those granules should be regarded as Bacteria I know not. The particles found in vaccine lymph are not at all similar to the organism found in putrefactive fluids (see fig. 4 & 5.) Two of these particles may be seen to approach each other and to adhere as if they were surrounded by a glutinous substance. When they meet their movements are very active and semi rotatory in character, and quite different from the oscillatory movements of the septic bacteria. The vaccine particles also come together so as to form larger clusters, and these also have a semi rotatory motion. I have seen a cluster consisting of about a dozen of these particles rotate rapidly upon its axis like a *volvox globator*. The mode in which these particles cluster, and their manner of adhering to each other as if their surfaces were glutinous, remind one forcibly of the clustering of the white blood corpuscles. They are in every respect more similar to the granules of white blood corpuscles, or of pus corpuscles, than they are to Bacteria. These particles do not elongate into rods. I have examined lymph kept in closed tubes for two years, and the particles were in every respect the same as those of fresh lymph. If care, however, is not taken to exclude air and external germs from the lymph in sealing the capillary tube, it may be noticed that the fluid becomes turbid at

both ends. When it is blown out upon a glass slide, and a cover laid upon it, the drop is seen to be surrounded by a rim of opaque substance like coagulated albumen. In this rim myriads of rod bacteria may be seen, whilst in the clear central part of the drop the vaccine particles may be observed without any admixture of Bacteria. The genesis of the rod bacteria at the two ends which had been in contact with air, shows that they have been generated from germs introduced from without; whilst the fact that they only invade the lymph by degrees, as they would any other albuminous fluid, tends to show that the Bacteria and vaccine particles are essentially different in nature. I read a paper on this subject before the British Association at the Bradford meeting, and was glad to find Dr. Burdon Sanderson acknowledge in the discussion which followed, that his own mind was not yet made up whether to regard the vaccine particles as organisms or not.

I would ask Dr. Burdon Sanderson, and other believers in the germ theory, to keep their minds equally free with regard to the particles found in the fluid of small pox pustules. This winter I have made a large number of sections of small pox pustules. When taken from the body twelve hours after death I have found plenty of rod-bacteria in the pustules. But through the kindness of my friend Mr. Needham, I obtained a pustule from a living body on the sixth day of the eruption, and I mounted over half a dozen sections, but could not meet with rod-bacteria. I have examined many capillary tubes of lymph taken from the pustule at different stages, in association with my friend Mr. Broadbent, but we did not meet with any rod-bacteria. We uniformly met with fluid having all the morphological characteristics of vaccine lymph up to the fifth or sixth day; but as the pustule approached maturity the fluid in addition to the particles became filled with pus corpuscles. Fig. 3, is a drawing of lymph from a small pox pustule on the eighth day. But what of the organisms which Dr. Klein found in the lymphatics? I could not find them in any of my sections. The appearances figured by Dr. Klein were however pointed out to me. On expressing some doubts as to the accuracy of Dr. Klein's results to Mr. John Priestly,

assistant to the Professor of Physiology, at Owen's College, a gentleman who has studied under Dr. Klein, he told me that he could show me these appearances. I have taken this drawing from a slide given to me by Mr. Priestly, and although it is somewhat rough, you will, I think, at once recognise its extreme similarity to that figured by Dr. Klein. (The drawing was shown to the section but it is not reproduced here.) You will be surprised, however, to hear that the slide in question was not taken from a small pox pustule; but from the intestines of a rabbit poisoned by a vanadium salt. Previous to the section being cut the intestine was hardened by the method adopted by Dr. Klein, and also mounted according to his method, and these appearances, whatever they may be owing to, were not once only, but very frequently met with. Mr. Priestly thinks, and I quite agree with him, that these appearances do not represent any organisms, but are due to the mode of preparation or mounting. Prof. Tyndall may now write a letter to the '*Times*' to announce the important fact, that the specific organism of small pox has been found in the intestines of vanadium poisoned rabbits. The arguments that I have hitherto advanced in favour of the view, that the particles found in infective fluids originate in the tissues of the body have been derived from a consideration of their form; but equally cogent reasons might be derived from a consideration of their function. I do not deny that the introduction of Bacteria into wounds and abscesses increases the danger of pyæmic infection, and hence I admit to the fullest extent the efficacy of antiseptic treatment. When Bacteria gain admission into fluids only partially in contact with living tissues, such as the fluid of an abscess, or a collection of fluid in the serous cavities, septic changes are set up just as septic changes are set up in putrescible fluids outside the body. When these changes are produced they excite a new inflammatory action, and help to disintegrate the surrounding tissues and the pus corpuscles; but I regard the products of this disintegration as themselves the cause of the increased infective power. This view would place the theory of pyæmic infection in analogy with the infection of tuberculosis and cancer and other pathological tissues, and not with the invasion and progress of a

parasitic disease. An albuminous fluid exposed to the atmosphere will undergo putrefaction nearly as readily in the pure air of the country as in the polluted air of an injected hospital; but surgical cases do not undergo the same risks when operated upon in the former as in the latter circumstances. This shows that some element over and above the presence of putrefactive Bacteria must exist in order to constitute an infected atmosphere. If this element should consist of small living particles derived from the tissues of the body, what is called the antiseptic treatment will be equally efficacious against them as against bacteria; hence the germ-theory, although false, will so far lead to good practice.

There is another well known fact which shows how widely different is the power of setting up changes in other individuals possessed by the particles found in infective fluids and by the organisms which are concerned in the putrefactive process. After death from some diseases, such as Erysipelas and Peritonitis, the blood and tissues are found to be crowded with these so-called micrococci. Anatomists are well aware how infectious such cases are; but in a few days after death, when putrefactive changes set in, the micrococci and zooglea and bacteria termo multiply in myriads, but instead of the infection becoming intensified it soon ceases altogether. This shows that the particles found at death, whether micrococci or not, have very different physiological powers from those which are present when putrefaction sets in.

The foregoing paper was read at the meeting of the British Medical Association at Sheffield, this year, and I reproduce it here in the exact form in which it was read, because I did not know at that time anything about Dr. Creighton's communication to the Royal Society, and cannot therefore be supposed to have been influenced by his paper. I mention this circumstance because I am anxious to claim a share to Mr. Priestly, of Owen's College, of whatever credit may be due to the detection of the error into which

Dr. Klein had fallen in his interpretation of the appearances seen by him in his microscopical examination of sheep-pox. I exhibited a drawing at Sheffield from one of Mr. Priestly's slides, which corresponded very closely to Dr. Klein's delineation of the so-called small pox organisms. With regard to Dr. Klein himself, I may be permitted to say that the scientific reputation of the author of that elaborate work, 'The Anatomy of the Lymphatic System', can well bear the strain of his being proved to have formed a wrong judgment on a subject in which it is proverbially difficult to form a right judgment. Dr. Klein's drawings of the appearances seen by him were so faithful as to render the error of interpretation into which he had fallen comparatively easy of detection. When once it was noticed that similar appearances may be seen in transverse sections of the vessels, especially the lymphatics of both healthy and diseased tissues; that they are remarkably distinct in crupous pneumonia, and also in a fibrinous clot from the heart after death; and that they may even be seen in healthy blood, and particularly in inflammatory, and pyæmic blood it could not long remain doubtful that the appearances in question were not organisms at all but stood in some relation to the coagulation of fibrine. I would remark, however, that when Drs. Burdon-Sanderson, and Klein, both of them as capable as any men living to form an opinion on a subject of this nature, have fallen into what now appears to have been a transparent fallacy, less considerable men ought to profit by the lesson. And when other men endeavour to show, as I have done for years, that the particles of infective fluids are not proved to be organisms at all; the assertion should be met by arguments, and not by an array of the opinions of "competent observers." When "competent observers" employ the logical method of agreement in forming their opinions they are liable, like "incompetent observers," to fall into all the fallacies incident to an imperfect method of research; and whatever value may be attached to the observations themselves, no disrespect is meant when we refuse to attach any inordinate value to the judgments formed respecting the observations:

The paper which I read before the physiological section at the

Meeting of the British Association, at Bradford, in 1873, was entitled "Microzymes as partial Bionta." Microzyme was a term introduced by Dr. Burdon-Sanderson into this country after Béchamp, as a generic expression for all those lowest organisms found in putrefactive fluids. It did not take root in this country, and even Dr. Burdon-Sanderson himself has abandoned it for the more common and usual term—Bacterium. Bion is a word used by Hœckel to indicate a physiological in contradistinction to a morphological individual. He divides Bionta into three kinds:—

1. The Actual Bion, being the fully developed and completed individual.
2. The Virtual or Potential Bion, including all stages of a developing organism before the stage of the completed individual is reached. And
3. The Partial Bion, including all parts of organisms which have an independent life when detached from the parent organism, but which do not develop into higher forms.

According to this classification, a white blood corpuscle is a partial Bion. It possesses independent life, as shown by its movements, after its detachment from the parent organism, but does not develop into any higher form. My contention in this paper was, that the living particles found in infective fluids were similar in nature to the white blood and pus corpuscles, and therefore were partial Bionta. In the discussion which followed, the opinions of "competent observers" were showered on my head. To this I could only reply that Dr. Beale was a "competent observer," and that he held a similar opinion to the one I advocated. Since that time other "competent observers" have come forward on the same side. In a very valuable paper by Drs. Moxon and Goodhart, in Guy's Hospital Reports, which I have seen for the first time a few days ago, (Third Series, vol. xx. 1875,) a similar conclusion is arrived at. The paper is entitled "Observations on the Presence of Bacteria in the Blood and Inflammatory Products of Septic Fever, and on the Cultivation of Septicæmia." But considering the results obtained this title would have been more appropriate

had the authors substituted the "Absence of Bacteria," instead of the "Presence of Bacteria." The authors say, (p. 234,) "With regard to the origin of these granules (the context shows that these granules were the bacteria found by other observers) nothing can be said with certainty, but bearing in mind the appearances and behaviour of the blood corpuscles in the febrile states in which we have examined them, how beaded the coloured ones become, and how granular are the colourless ones, and what activity of amœboid movements the latter at times display, we think that their presence is quite accounted for by disintegrative processes, consequent on changes going on within the body." To this I will add an observation I made that the white corpuscles of tadpoles and of very young frogs are much more granular than those of old frogs. This would seem to indicate that the granular condition of the white corpuscles bears some relation to the active multiplication of the units of the body, which proceeds during active growth. Dr. Beale has shown that during the febrile state the protoplasm of the entire body is increased; a condition which indicates a decrease of the structure of the body and an increase of its protoplasm probably obtained by rapid multiplication of the white blood corpuscles. For several years I have examined, as opportunity offered, infective fluids and recently the blood in septic cases, and I can confirm almost every observation made by the authors of this article. I can confidently confirm the truth of their statement (p. 239,) when they say, "With regard to the observations on the contents of abscesses we should say that as a rule abscesses being away from the possibility of atmospheric influence, nothing besides granular matter will be found, sometimes more sometimes less." But pyæmic pus as already noticed differs from common "laudable" pus in other respects than in containing a greater amount of granules. The corpuscles of the former are not so large as those of the latter, while they are more granular. In infective pus very few full sized pus corpuscles will be seen, the largest proportion of them are about half the size of the corpuscles generally figured in text books, while there are present all intermediate sizes down to the free granules.

This condition would appear to arise from the diminution of bulk which must accompany rapid multiplication of units.

While conducting my observations it has often occurred to me that although the granules in infective fluids might be derived from white blood corpuscles; yet that when withdrawn from the influence of the living tissues they might give rise to Bacteria. Many believe that this limited kind of heterogenesis does take place; who, at the same time refuse to believe in the reality of the wider kind of heterogenesis which consists in the origination of the living from the not living. Drs. Moxon and Goodhart appear to believe in the reality of this limited kind of heterogenesis. At page 237, they say:—"The various observations we have mentioned "which would amount if fully verified to proof of the development "of vegetable organisms from the molecules of which we are speaking might be regarded as evidence of transformation from an "animal to a vegetable matter, if so be that we hold the white "corpuscles to be an animal albuminous substance." I examined fluid drawn from the inflamed thigh of a frog and found that some of the granules comported themselves very similar to the vaccine particles, while at the end of an hour others were seen to be arranged in rows of three, similar to bacteria *termo*, but motionless. Part of this fluid was kept sealed in a capillary tube and examined three days afterwards; the granules, which formed groups like the vaccine particles, were still to be seen; while those which were arranged in rows appeared to be replaced by very actively moving *Filaria*. The observation of disintegrating pus and white blood corpuscles after being separated from the body is equally suggestive of such a transformation. But as I have watched the appearances in question as readily in a clot from the heart after death from *Leucocythæmia*, and in pus from the bladder in a case of chronic cystitis, as in pyæmic pus, it will be understood that this question has no special connection with infective processes. In fig. 6, is represented pus from the bladder, showing the corpuscles as they appeared when examined at various times after the urine was voided. At (a) is shown a corpuscle as it appeared twelve hours after. The corpuscles were somewhat larger than

similar corpuscles when examined immediately after their separation from the body, and appeared to have been surrounded by a more distinct membrane. The granules were also more distinct and exhibited Brownian Movements. At (*b*) the corpuscle is depicted as the average of them appeared twenty-four hours after the urine was voided. The corpuscle is still larger and the granules have become arranged in rows very similar to the bacteria which were seen in large numbers moving in the fluid between the corpuscles. At (*c*) is represented a corpuscle in which the nucleus was particularly distinct. The granules had become arranged longitudinally like those at (*b*), but in addition, the granules within the nucleus presented oscillatory movements similar to those of the bacteria. Examined thirty-six hours after the urine was voided, (*d*) represents a corpuscle from which the granular contents have escaped, and nothing but a flattened membranous disc is left. At (*f*) a corpuscle is shown where the greater part of the granular contents may be seen in the act of escaping; while at (*e*) is seen one where part of the granular contents have escaped, and part is left behind. A colony of bacteria is represented at (*g*) as it appeared 12 hours after the urine was voided. The arrangement of the granules appears in every respect similar to that of the granules within the corpuscles examined at the same time; while at (*h*) is represented a colony as it appeared thirty six hours after the urine was voided. The granules in the latter are arranged more in rows than in the former, and some of the rows at the edge were seen to have an oscillatory movement and at last to break off from the mass and to move about in the fluid as free bacteria. It may be said that the appearances presented by the corpuscles were due to some of the free bacteria adhering to their exterior and that they were seen through the semi-transparent corpuscle instead of being within it; but I assured myself that this was not the case by putting a drop of water on the slide at the edge of the cover glass, and when it began to enter under the cover, it produced a movement of the corpuscles causing some of them to roll over. When this pus was examined on the third and fourth day after the urine was voided the only remains of the corpuscles to be seen

were those flattened pseudo-membranous discs, while the granules and colonies were almost entirely replaced by free bacteria and allied organisms. I say pseudo-membrane because the corpuscles when fresh have no real membrane at all; and when after three or four days we find that the perishable protoplasm is still represented by those discs; the most ready inference is that the granules which may be supposed to be more durable have passed bodily to form bacteria without having undergone disintegration. But the most ready inference in this as in many other cases may turn out to be the wrong one. And if I were from these observations to speak dogmatically upon this point I should be falling into the error which I have already condemned in others. So far, I have only been able to apply the logical method of agreement, and the conclusion cannot be regarded valid until the application of the logical method of difference. Could I show that there was no other possible way of accounting for the presence of bacteria and the disappearance of the granules in these corpuscles except by direct transformation of the one into the other, then the process of heterogenesis would be proved to be real. But this negative side of the proof is totally wanting. In the fluids examined by Drs. Moxon and Goodhart it was quite possible, and in those examined by me almost certain, that the germs of Bacteria had gained access to them from without; and if so, either the bacteria or their germs might have penetrated into the substance of the corpuscle. There is every reason to believe that in the first stage of life the bacteria cannot be distinguished morphologically from the granules of pus corpuscles, and if this is true the pus granules would be so mixed up with bacteria that there would be no means of isolating them and watching them while undergoing disintegration. Hence I am inclined to think that the pus granules are not transformed directly into bacteria, and this conclusion is much strengthened when we remember that if vaccine lymph is carefully sealed in capillary tubes it may be kept for years without its particles being transformed into active Bacteria.

In conclusion I trust that I am not premature in expressing

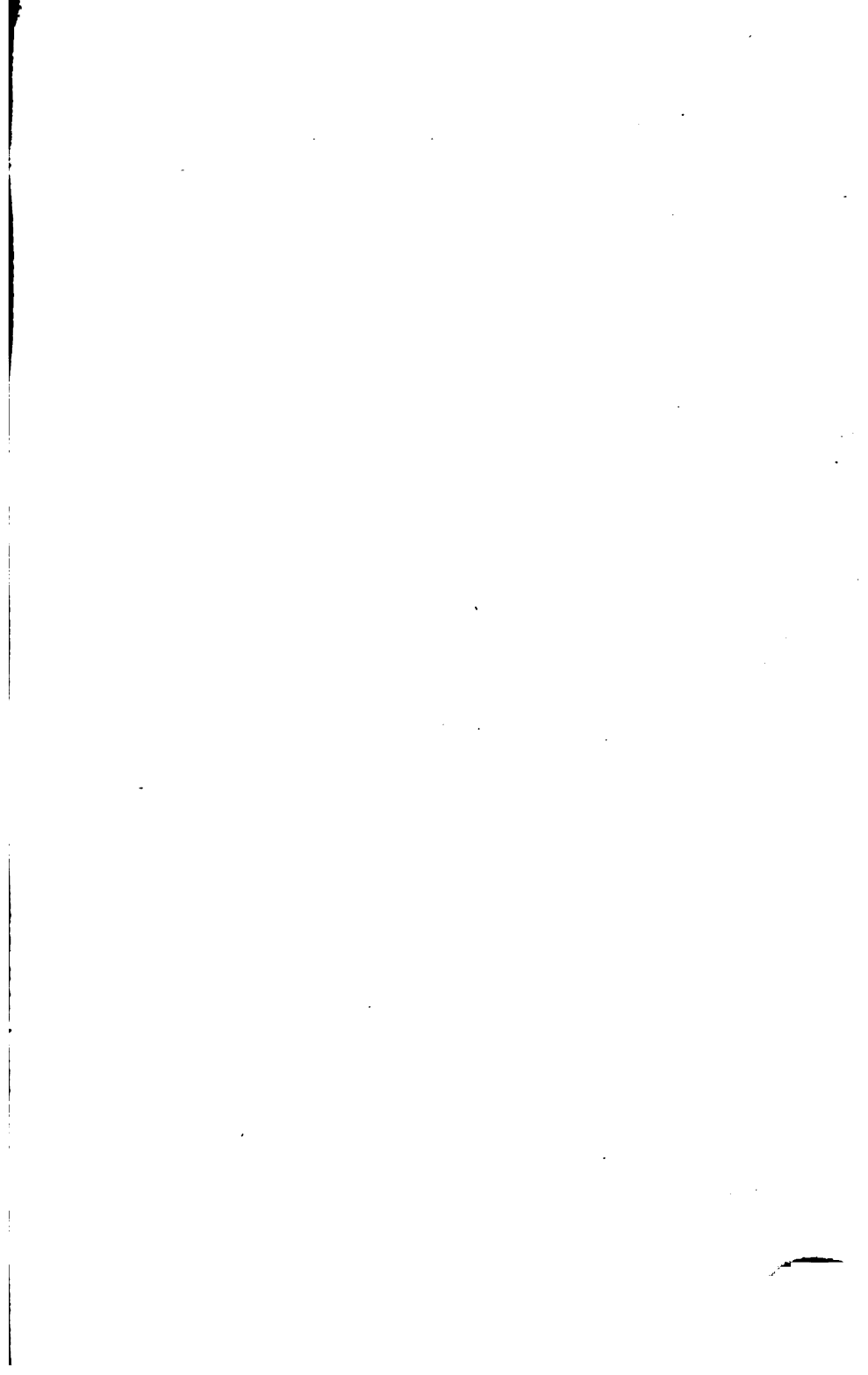


Fig. 1



Fig. 2

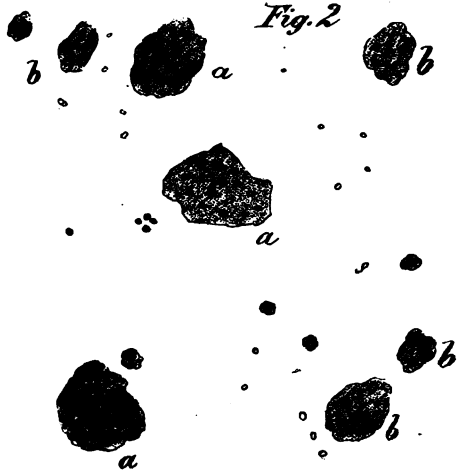


Fig. 3



Fig. 4



Fig. 5



Fig. 6

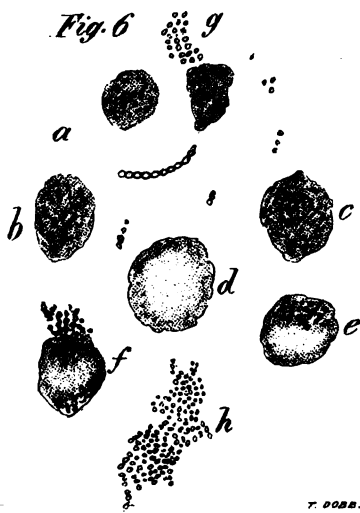
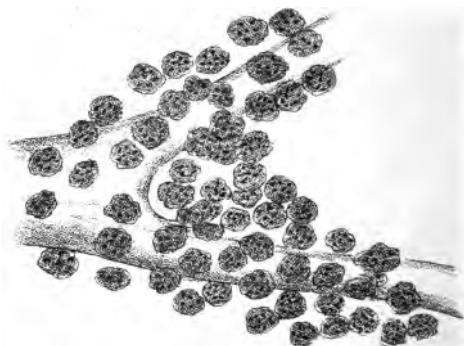


Fig. 7



the opinion that the Germ theory is beginning to decline in the estimation of thoughtful men. That it will by and by decline in the estimation of all men, whether thoughtful or not, I never entertained any doubt; because I never believed that the extension of this theory to the explanation of infective diseases had any solid foundation; and at any rate I see no reason at present to induce me to desert my first love,—"The Graft Theory of Disease."

Explanation of the Plate.

Fig. 1.—Injected two minims of strong liquid Ammonia into the muscles of the thigh of a frog at 8 p.m., August 25th, 1873. Thigh much swollen on the evening of the 27th.

Microscopic examination with 1-12 immersion objective and Creyepiece of fluid withdrawn from thigh by a capillary tube after puncture, showed (*a*) altered red blood corpuscles, (*b*) nearly full sized white blood corpuscle—(*c, c, c*) fragments of white blood corpuscles—numerous granules.

Fig. 2.—Fluid drawn from same frog as above immediately beyond the boundary of the intense inflammation showed (*a, a*) full sized white blood corpuscles—(*b, b, b*) Fragments of white blood corpuscles—Granules.

There were no marked changes in the mesentery of this frog. Magnified 800 diameters.

Fig. 3.—Fluid from small pox pustule 8th day of eruption examined two hours after being taken from the body. *a*. Corpuscles of various sizes. *b*. Groups of granules.

Fig. 4. Bacteria from putrefactive fluid.

Fig. 5.—Vaccine Lymph—(*a*) Groups of granules—Free granules.

Fig. 6.—Pus from the bladder. (*a*) Examined 12 hours after the urine was voided; (*b*) examined 24 hours after being voided—the granules appear to be arranging themselves into bacteria; (*c*) examined 36 hours after being voided—granules similar to (*b*), nucleus very distinct—granules within it were similar to bacteria termo; (*d*) Flat disc consisting of the outer covering of the corpuscle from which the contents have escaped; (*e*) The same with some of the contents still left; (*f*) One from which a large part of the granular contents may be seen escaping; (*g*) Groups of granules found 12 hours after the urine is voided; (*h*) A similar group 36 hours after, in which the granules are becoming arranged similar to the staff shaped bacteria.

Fig. 7.—Vessel from mesentery of frog four days after injection of strong liquid ammonia into the thigh showing the distribution of the white blood corpuscles (1-5 objec.) around the walls of a vessel, 450 diam,

SOME RESULTS OF LEAD-IMPREGNATION.

By Dr. SHEARES, Liverpool.

CASE 1.—Case of Plumbism resisting equally Iodide of Potassium, Anodynes vegetable Purgatives, but speedily yielding to purgative doses of Sulphate of Magnesia : self-adopted prophylactic measures, viz. ;—Chewing tobacco, and taking Codliver oil.

THE first case I shall mention is that of a House Painter, æt 30, who had been subject for some weeks, before I first saw him, in April, 1876, to periodic attacks of acute pain in the region of the transverse colon with constipation and the peculiar waxen pallor characteristic of subjects of Lead poisoning. He had a feverish furred tongue, which, however, continued always moist, and the circulation was depressed, pulse 54, as well as his animal spirits to a remarkable degree. The characteristic blue or purplish black line was well marked along the gums at their junction with the teeth, and the teeth themselves along the corresponding line were much blackened and 'holy.' The interesting points in the case are that the administration of the Iodide of potassium in full doses, together with the ordinary combination of sedatives and purgatives gave no effectual or abiding relief until he was ordered the sulphate of magnesia in 2 dram doses every morning for a week, after which he was cured of the complaint for a period of 5 months.

On the 14th Sept., he returned suffering as before from severe abiding pains in the epigastric region, and that of the transverse colon, the tongue moist with a brownish fur as before, appetite nil, bowels constipated. He was immediately relieved by a few purgative doses of sulphate of magnesia followed up by the Iodide of potassium mixture and codliver oil.

This man adopts of his own choice two methods of keeping the lead out of his system for both of which something may be said. He chews tobacco and he takes Codliver oil, the former insuring the rejection of any floating particles of leaden dust.

which may get into his mouth and nose by spitting, and the latter which he is positive "gives him ease when the pains seize him" probably acting as a solvent for the lead, uniting with it within the body to form a kind of lethargic plaster and as such eliminated. In Sir Geo. Baker's Classical treatise it is mentioned that there is a current belief amongst the people of the pottery districts of Styria that the common use of butter, bacon and other fatty and oily substances insures perfect immunity from lead poisoning.

CASE 2.—Chronic Gastrodynia not relieved by the usual treatment but yielding at once to purgative doses of Sulphate of Magnesia followed up by Iodide of Potassium, attributable to Lead-poisoning.

The 2nd is a case of Gastralgia in a coloured man and Ship's cook of 2 years standing, accompanied with loss of appetite and strength, constipation and vomiting of food, and this indifferently whether at sea or on shore, and resisting all the ordinary remedies, such as Bismuth, Hydrocyanic acid, Alkalies, and Sedatives. Of laudanum he had taken great quantities with only temporary relief. He suffered from attacks of severe pain in the lower limbs (a symptom of plumbism) in addition to the Belly-ache and had a more or less distinctly marked blue line at the junction of the gums and teeth.

I failed to apprehend the true nature of this case for a period of 3 or 4 weeks, the symptoms being mainly referable to the stomach, and but that I had under treatment at the same time a genuine case of lead-poisoning with anomalous symptoms I might have failed altogether to give this patient any relief. In accordance with the theory of lead-poisoning I now administered a full dose of Sulphate of Magnesia, followed by a warm bath, and continued doses of the Iodide of Potassium.

The abdominal pain which 'had not left him for many months' disappeared as if by magic and he kept well till the following spring—a period of 4 months, when he had some attacks of abdominal pain combined with Tertiary ague fits contracted abroad. A combination of Epsom Salts and Quinine effected a rapid and effectual cure.

Analysis of the fæces in this case would probably show that the lead is eliminated from the system as an insoluble sulphate, and it is probably in virtue of their cathartic action, stimulating the sluggish intestine and unloading it of detained irritant gaseous and solid matters, at the same time that it converts into an in-

soluble and comparatively innocuous substance any particles of lead they meet, that the Sulphates of Magnesia, Soda and Potash possess so manifest an advantage over the Iodides in the treatment of acute cases.

CASE 3.—Inveterate case of Chronic Lead Poisoning with general symptoms, of acute Pleurisy and Phthisis rapidly relieved and finally cured under the use of the Iodide of Potassium, &c. Proofs of elimination of lead in the urine and sweat.

Edward G—, at 28, by trade a Japanner, a good deal below the medium height, accustomed to work in a close heated atmosphere. Has suffered from several attacks of Painter's Colic, and last Summer (1875) when treated for the same had a distinctly marked blue or bluish black line along the gums. He was cured by means of Saline purgatives and Iodide of Potassium but never seemed to get into vigorous health though always able to follow his employment.

He had a severe Quinsey in the autumn and a certain tightness and swelling about the ascending ramus of right half of lower jaw which slowly disappeared.

In the month of February 1876, he complained much of Dyspepsia and wandering abdominal pains together with general debility and a waxen pallor of the countenance for which he obtained considerable benefit from a combination of iron and Belladonna. The anæmic pallor remained and being caught one day in a rough, snowy, blast as he was returning much exhausted from his work he was struck with a Pleurisy (?) and treated for the same for some time by his Medical attendant. He was very speedily reduced by this attack to what seemed to his relatives to be the last extremity and he was pronounced to be in a rapid decline. I found him in a state of the greatest suffering. Any but the very shallowest inspirations were followed by the most acute and distressing lancinating pains in the left side, he had a troublesome cough, he could not lie down in bed, his skin was bathed in perspiration by day and the night sweats were still worse, he wore the most haggard, pallid, anxious expression of countenance, he had lost flesh and though the temperature was not high the pulse was running day by day at the rate of 120 per minute. Altogether a most serious looking case.

Still on making a Physical Examination of the chest, and the slightest force in percussing it caused him to wince like a hysterical girl, I could find absolutely none of the true signs of Pleurisy. On the contrary, the percussion was everywhere resonant, there were no friction sounds and the air entered the left lung sufficiently freely. And the family history was good.

I formed the conclusion that this was no common case of Pulmonary or pleuritic disease but rather a peculiar neuropathic lesion or State of Hyper-

æsthesia of the nerves of the left side of the chest simulating Pleurisy, the result of cold acting upon a system anæmic and debilitated through malnutrition, and this consequent upon the impregnation of the tissues with a mineral poison—Lead. At this stage his nose bled several times: the blood readily clotted in the nostril, but was quite pale as if deficient in red globules.

I ordered a large sheet of Spongio-piline to be kept constantly applied hot with Laudanum to the side and an anodyne and depurative mixture.

Tinct Belladonna 3 drachms.

Potassii Bromidi 1 drachm.

Potassii Iodidi 2 drachms.

Syrupi Lemonis 1 ounce.

Aq: Menthø 8 ounces.

Sig. :—A dessert spoonful four times a day.

March, 18. Patient passed a delightful night—the best since the commencement of the attack notwithstanding Opiates, &c. He had 8 hours of uninterrupted sleep and the pulse which had been running for some days at about 120 fell to 100 and to 96 before noon. All this was consequent upon taking 2 doses of the mixture,

On the 20th he complained of a kind of Lumbago pain and on the 22nd his convalescence was interrupted by some domestic trouble which threw the nervous system into an unwonted state of excitement.

By the 30th of the month all cough and pain, and all the night sweating had ceased, his appetite was returning, the bowels acting naturally and his sleep at night was undisturbed and profound. The pulse continued about 90 and the lower gums were now quite fair and free from the lead-line, while the upper gums though clearing up were not yet quite free from the bluish black line. He continued taking the above mixture, Sulphuric acid drink ad-libitum, an occasional dose of Sulphate of Magnesia and an occasional sulphur bath. 4 ounces of the Sulphuret of Potash were dissolved in enough hot water to form a sitz-bath and the patient was thoroughly sponged at night. An immense bulky sediment of black Sulphide of Lead formed at the bottom of the bath on each occasion but a portion of this was evidently derived from the paint.

The blood stains on the patient's pocket handkerchief also acquired a smoky hue when steeped in a solution of the sulphide of Potassium and the urine when clarified by passing through Charcoal and allowed to filter down upon a fragment of the Sulphide, instead of forming a whitish milky fluid as in healthy urine produced an olive-green solution from which was at length precipitated a copious black sediment of Sulphate of Lead.

In order to make quite sure that Lead was actually eliminated from the system I had a portion of the urine sent for Chemical Analysis to Mr. Edward Davies Analytical Chemist whose results entirely confirmed my own.

The fact is beyond doubt therefore that this patient's blood and tissues were charged with lead in such manner that nutrition and the healthful action of the nerves and muscles (specially the intercostal and phrenic nerves and the involuntary muscular fibres of the great intestine) were interfered with and that the administration of the Iodide of Potassium gave immediate relief and that most probably through its solvent and eliminative action over the mineral poison.

The discolored line along the gums had quite disappeared in May and his health has been since thoroughly re-established.

M. Melsens who in 1849, was the first to point out the value of Iodide of Potassium in Chronic Lead-poisoning alleges that neither sulphuric acid nor the sulphates can be considered as remedial agents in chronic diseases arising from handling preparations of lead whilst the sulphate of lead is a poison sufficiently virulent to destroy animals in a few days. On the other hand when sulphate of lead and Iodide of Potassium are conjointly administered to a dog no morbid effect is produced.

In cases of mercurial tremor the mercury came away in like manner as an Iodide and as such appeared in the urine.

Dr. Anstie and many others who might be quoted confirm the statements of M. Melsens that Iodide of Potassium in 10 to 20 grain doses, three times a day effects, a real elimination of lead by the channel of the urine. Dr. Parkes, Dr. Sieveking, Malherbe, and Fletcher of Dublin, all agree that the kidneys are instrumental in removing the poison under the influence of Iodide of potassium. Dr. Begbie considers that the skin is the chief emunctory having failed to obtain evidence of lead in the urine in 2 cases of Gouty Rheumatism with Chronic Plumbism.

In the '*Dublin Hospital Gazette*,' for March, 1872, two cases are related of obscure Neuralgic pains produced by lead in the system and announces the following method of detecting the presence of this metal. After administering the Iodide of Potassium for a short time the patient is desired to place a small piece of sulphuret of Potassium, tied up in a rag, in the urine each

time he passed it. The Iodide of lead in the urine is decomposed and sulphuret of lead formed which remains in the rag.

Kussmaul & Maier relate a case of a house painter of 35 who had suffered for several years from chronic lead poisoning, showing itself in an anæmic (bluishyellow) colour, emaciation, dyspnœa, constipation, and abdominal pain, and towards the end of life in attacks of colic, with dyspnœa, and slow pulse. He was cut off by an attack of vomiting, with profuse diarrhœa, and collapse. The temperature was only slightly above the normal at any time. He had no paralysis or brain—symptoms. The autopsy revealed Chronic Catarrh of the Mucous membrane of the stomach, intestines and ductus choledochus, fatty degeneration of the glands of the stomach, atrophy of the Mucous membrane of the jejunum, ileum and upper part of the colon, atrophy of the intestinal villi, the glands of Lieberkuhn the solitary glands and Peyer's patches: fatty degeneration of the muscular coat of the intestines, pigmental degeneration of the muscular fibres of the heart.

M. Paul in the '*Archives Gen. de Med.*' gives some cases showing that Lead kills the foetus in utero, causing abortion: and it is possible that there may be such a thing as an *inherited Saturnine Cachexia* for the child of Case 3, had the characteristic waxen pallor and died in Convulsions of a fearfully prolonged character when about 7 months old.

CASE 4.—*Recurrent Paroxysmal Asthma, in a case of Chronic Plumbism followed by an attack of Acute Mania :—*

Robert Jones, aged 32, Plumber & Painter, had been complaining for 9 months past (October, 1865,) of morning sickness and vomiting after breakfast, constipation, colicky pains in the abdomen, loss of colour, &c.

About 3 months ago he began to have attacks of difficulty of breathing, and these have become gradually more frequent and severe until a month ago when they reached their greatest severity. The worse attacks last from 12 to 24 hours, and during their continuance he can not speak or answer questions, but frequently talks deliriously and labours for his breath like one on the point of suffocation while no air appears to enter his lungs. Nothing amiss can be detected with either

the Cardiac or Respiratory organs in the intervals between the paroxysm and there is never any cough or spitting. Sometimes he appears to his relatives particularly queer and 'soft.'

I came to the conclusion that this man's system was impregnated with 'Lead poison,' from a consideration of the following circumstances :—The appearance of a *blue-line* along the margins of the gums, the pallor of the countenance and lips, the colicky symptoms, the constipation, and his peculiar occupation. The question was whether we should not also trace the Asthmatic paroxysms to the influence of lead inducing spasm of the bronchial muscles as it does in those surrounding the large intestine? The Asthmatic Seizures gradually wore out and disappeared under the use of Iodide of Potassium with Antispasmodics and Chlorodyne, but towards the end of the month of October, his friends noticed indications of 'silliness' or 'softness' and in the month of March, 1868, he had a decided attack of Acute Mania for which he was sent away to an Asylum.

It appears therefore (and the fact has been shown necroscopically) that lead may be deposited in any or all the organs and by its presence seriously derange the organic life and functions of those organs. It cannot be doubted that its presence in the blood leads to a rapid destruction of the blood globules whence the pallor and anomia so commonly noticed, and in this case moreover to spasm of the Bronchial Muscles as evinced by the Asthmatic Seizures, and depreciation of intellectual power and at length Mania through interference with the due nutrition of the brain.

Garrod relates the case of a man, in whose cheek a leaden bullet had lodged giving rise to Plumbism and Mania.

Tronpeau mentions the case of a man dying of Eclampsia after having worked in lead for seven weeks. A notable quantity of lead was found in the brain, the spinal cord and the liver. He had been a great absinth drinker.

This is called by Dr. Oastie, the 3rd grade of nervous poisoning by lead—impregnation acting on the central nervous system. It may be preceded by no other symptoms than those of the first grade, viz :—Colic and constipation. Usually it is announced by the occurrence of Epileptiform fits which may either occur a number of times, with intervals of comparative health or the patient may almost immediately pass into a state of coma from which recovery is rare.

Dr. Edward Murphy, of Indiana relates a remarkable case of Lead-poisoning in a merchant, from the habit of chewing leaden bullets or shot when he went out shooting and which passing through the preliminary stages of Enteralgia and paralysis of the fingers, was finally attended with Epileptic convulsions of the most alarming character, complete amaurosis, excruciating pain in the testicles, soles of the feet and balls of the thumbs, with finally mental despondency threatening suicide—all of which ghastly train of symptoms gradually yielded before the exhibition of small doses of mercury, long perserved in. (In the delicate chemistry of the living body, are we at liberty to think of a metallic amalgam being formed between the two metals, of a more powerful affinity between the Lead and the mercury than between the Lead and the tissues, or are we to explain the cure simply by the well known fact of the wonderful absorbent and secretive and excretive—in short stimulating action of mercury upon the system.) This case would itself alone afford all the materials for a complete history of the toxicological effects of lead on the human system:—Neuralgia of the bowels arms and hands, legs and feet, testicles and sides, constipation, cachexia, convulsions, amaurosis, paralysis, mental imbecility, threatened mania !

Heubel found that analysis of the viscera of four dogs which died under symptoms of Eclampsia Saturnina showed that the bones contained the largest quantity of Lead, next the Liver and Kidney, then the central nervous system and lastly the muscles intestines &c. He attributes the obstinate constipation and colicky pains, not at all to the pressure of the contracted muscular fibres on the ends of the sensory nerves, but to this abnormal irritation of the Splanchnic nerve which inhibits peristaltic action. If the peristalsis were really increased in colic it ought to lead to diarrhoea rather than constipation. To the irritation of the Splanchnic he also attributes by reflex action the slowing of the pulse and the diminution of the urine observed during the attacks. The rapid atrophy of the muscles in lead-poisoning he attributes to general interference with nutrition which lead causes in a marked degree, and partly to the paralysis itself. The nerve tissues have a

special affinity for lead. To its action on these, both in their central and peripheral parts, he attributes the symptoms of chronic saturnine poisoning, and the repeated attacks he regards as caused by the occasional absorption of the lead into the blood from the organs in which the poison is deposited. Encephalopathia saturnina he attributes to the direct deposition of lead in the brain. Eclampsia saturnina he refers to capillary anæmia of the brain consequent upon œdema or a uræmic condition from accumulation of the urinary constituents in the blood.

CASE 5.—Abstract of a Case of Constipation, Albuminuria, Convulsions, Paralysis of fore arms and hands, and partial loss of Vision from Lead-poisoning, under the care of Dr. Wallace.

Mrs. A. æt 33, was suddenly seized with Epileptiform attacks in August, 1875. She had been under my care, with dyspeptic symptoms occasionally for the previous two or three years, the chief feature of which was persistent Constipation, and ultimately pain over the left lumbar and iliac regions accompanied with frequent Micturition. Albumen was found in the urine. The treatment was directed to regulating the bowels by proper diet and occasional aperients and the administration of Potassium Bromide and Chloral Hydrate to allay the convulsions. Several medical men saw her in consultation but no light was thrown upon the nature of the case further than that it seemed a case of Bright' disease. But it was remarkable in this respect that the albuminuria was not persistent, and its periods of recurrence corresponded to the menstrual periods. It was accompanied by most of the Symptoms of Eclampsia, such as vertical headache, disordered vision, pain in the left loin, and general malaise, with twitching of the orbicularis palpebrarum muscles, and starting of the limbs. The progress of the case developed other symptoms, such as Numbness and gradual loss of power of both fore arms, while the hands dropped and the power of using the fingers became less and less. This condition was attended with great pain, chiefly nocturnal. By and by vision became affected and at the distance of three to four yards, she could not distinguish any one. The pupils became enlarged, and remained so. The appetite was bad and she lost flesh rapidly.

This state of matters pointed to lead poisoning, and as the gums were becoming retracted from the teeth and had a faint bluish line pretty well marked, she was put upon the Iodide of Potassium and Magnesia Sulphate, and a sample of the urine was sent for analysis to Dr. Brown, of the Royal Infirmary School of Medicine, but only a faint trace, hardly appreciable, of lead was found. However, treatment was persevered with, as a distinct amelioration of the pains followed. Champooing and warm baths formed the remainder of the treatment.

This method of treatment was continued from the beginning of October, 1875, to March, 1876, by which time the symptoms had entirely disappeared, with the exception of the recurrent albuminuria, but by June that had also disappeared. She now undertook her usual household duties, and continued well until the middle of Feb. 1877, when some of the symptoms returned, such as loss of appetite, disordered digestion, constipation, pain in left loin, dysuria, and intense head-ache. The bluish line round the gums was well marked. The urine was highly albuminous—about a twelfth—by the addition of heat and nitric acid. Specific gravity 1010, a peculiar yellowish precipitate coloured the albumen thrown down. A sample of the urine was sent to Dr. Brown who reported lead in considerable quantities. I also sent two samples to Dr. Shearer, and he reports lead in quantity sufficient to account for the Symptoms. The former treatment was resumed, the bowels having first been well acted on by doses of compound Jalap powder, and a large quantity of dark fecal matter removed, of a very offensive character.

March 5 : she has gradually improved in every respect and is now able to be up and about her household duties, urine normal in appearance, Sp. gr. 1010, Albumen still present but in very small quantity. It is gradually disappearing, under treatment, precisely in a similar way to what occurred previously.

March 10th : Urine pale but less so and specific gravity higher 1015. Reaction still neutral, albumen diminished to 1-30th, violet colour developed by addition of nitric acid in the cold. The yellowish appearance of the precipitated albumen when the urine is boiled with Nitric acid is accounted for by the formation of Nitrate of Lead, itself a milk-white and opaque substance.

When the urine is filtered down upon a fragment of Sulphide of Potassium, and allowed to stand for 24 hours, it still develops an abundant brownish black precipitate of Sulphide of Lead.

Two points in this remarkable case require clearing up viz : the source of the lead impregnation, and the connexion of this, if any, with the albuminuria. On these points Dr. W. observes that every one besides is healthy in the house : that the cistern had been seen to repeatedly, and no defect discovered : that before her first attack, Mrs. A. had been under medical treatment for Endo-metritis and engorgement of the cervix, and that towards the end of the treatment when near complete recovery, she had used the Iodide of Lead pessaries per vaginam.* The case may be compared with that mentioned by Dr. Garrod, where the patient got the Lead into his system through his snuff box which was made of Lead.

When we remember that the Kidneys (with the Liver) are the organs, second in order, for the lodgement of the mineral poison, as ascertained in the experi-

* These pessaries had been used for a short time, four years previously ; and none had been used before the relapse. Besides, the vagina does not absorb metallic substances readily.

mental cases of Henle' and Heubel, and when we know that lead exerts a specific paralytic action both on pale and striated muscular fibre, we are not surprised that albuminuria should be one of the sequelæ of Lead-impregnation. The probability is that this condition would be found co-existent with other symptoms of Plumbism, in most cases, if carefully looked for. Paralysis of the muscular coats of the arteries of the Malpighian tufts in the Kidney, would necessarily lead to passive exudation of the blood serum with its soluble albumen: while a like arrestment of action in the secreting Renal cells would readily explain the uræmic condition with consequent Eclampsia, dependent on the retention of the urinary constituents in the blood.

*Influence of Lead-impregnation in causing Gout and
Rheumatism.*

Dr. Garrod first in 1854 showed that this form of metallic impregnation is a powerful predisposing cause of Gout. He was struck with the fact that a large percentage of the gouty patients which came under his care in hospital practice, consisted of Plumbers, Painters and other workers in lead. He found that 30 p. c. of the gouty patients in Hospital had been subjected to the influence of lead: that many had Lead-colic: that some had suffered from wrist-drop or other form of paralysis: that all exhibited the characteristic blue-line along the gums. He believes that individuals suffering from the gouty diathesis are more susceptible to the influence of lead than the majority of other people. In some instances severe attacks of Gout have been induced in gouty patients by the medicinal administration of Lead salts for Epistaxis or other Hæmorrhage. He found that the administration of Lead salts or the inhalation of the smell of Paint diminishes the secretion of Uric-acid, so that there is a diminished elimination of urea by the kidney, while the blood is found to contain an abnormal proportion of that substance. He found that the blood of individuals suffering from Lead palsy, always contains an abnormal amount of uric acid, and the same he alleges probably holds good in all cases of Lead-Colic. Lastly he states, there are many facts which seem to shew that the influence of Lead, when uncombined with that of *fermented liquors* is scarcely able to produce Gout.

None of these positions so far as I am aware, have been controverted by subsequent observers. Dr. Begbie of Edinburgh has shown that indulgence in distilled liquors, as whisky, on the part of persons working in Lead or even actual victims to its influence, does not create anything like the same proclivity to Gout

Dr. B. confirms Garrod's observations that Lead interferes with the uric-acid secreting function of the Kidney and so paves the way for an outburst of Gout or Rheumatism. He gives examples where the habitual use of beer was followed by Gouty attacks and the use of spirits by attacks of a Rheumatic Character—the arthritic attack in two cases culminating in Endo-carditis and Pericarditis. In the following case, the subject was equally accustomed to both beverages, that is to say he took beer or brandy indifferently, just as he could get it, but always in strict moderation.

CASE 6.—Recurrent attacks of Subacute or Gouty Rheumatism—more properly Irritative Rheumatic fever—in a subject of Plumbism.

A. S. J. P.—æt 50, a healthy, rather stout gentleman, possessing excellent digestive and assimilative powers and accustomed to little exercise, was attacked early in June, 1875 with Lumbago, muscular pains in the calves and shortly afterwards with articular Rheumatism of a severe type, with sour breath and perspirations, serious suffering, rapid pulse and sub-febrile symptoms. He is known to have suffered several attacks of Lead-colic and he is very frequently exposed to the inhalation of the smell of paint from living over large public offices, one department or another of which is being constantly repainted.

Relief was obtained, but no permanent cure effected by the use of opiates and alkalies, combined with Iodide of Potassium, warm baths, and purges of colchicum and colocynth; nor did the high pulse sub-side under full doses of Aconite, Bromide of Potassium, Veratium viride and Quinine!

The Temperature never rose above 100 degrees but the pulse-rate was for weeks together about 120 and was only momentarily affected by the remedies named. It was evident the patient was labouring under some source of irritation, some morbid virus in the blood, to which none of the remedies hitherto (unless we except the Iodide) had proved a real antidote. *I had yet to learn that a permanent high, irritable pulse, without cardiac complication and with the usual concomitants of Rheumatic fever is one of the sequelæ of Plumbism.*

At the end of a month he had chills, fever and a relapse, then we tried the plan of starving the system of azote by withholding animal food for a week but at the end of the 6th week, he had fresh chills and fresh joint-attacks, distinguished by a bluish or pinkish hue and great puffiness, but always without effusion into the joint.

By the end of the 2nd month there was an abatement of the pulse rate to 100, and some improvement in the general symptoms when he had another relapse, with inflammatory swelling of the right foot and ankle which yielded to colchicum, Potash and Quinine. Their effect was marked in a decided abatement of the pulse, the febrile heat and free flow of pale urine.

At the end of two months and a half, the virus was evidently slowly wearing itself out, under the influence of time and the Iodide of Potassium, with colchicum, and we sent him to the country for a change of air. Here he continued throughout cross and peevish, occasionally feverish and full of aches and pains but able to move about in a very lame and crippled manner: pulse fallen steadily to 90. The colchicum I think did good service, acting first as a diuretic and subsequently as a laxative, but the Iodide was probably the more important depurative agent, ridding the system of the latent cause of all his sufferings, viz., Lead, though unfortunately this was not reduced to demonstration by chemical analysis of the urine.

He was now able to return to his duties but in the 4th month he had a colicky attack. As already stated he is subject to these attacks, he has the bluish-white waxen pallor of subjects of Lead-impregnation, and he has had two attacks of Rheumatic fever with colicky symptoms and delirium previous to this! I could not absolutely state that the blue lead-line on the gums was wanting but it was not marked. He was quickly relieved of the colick and constipation by a dose or two of Battley's sedative followed by the sulphate of magnesia and the baths of sulphuret of Potash which yielded a bulky precipitate of the black sulphide of Lead. Unfortunately neither in this case were sufficient precautions taken to warrant us in stating that the Lead was derived from the patient's skin in whole or in part and not from the paint on the bath.

By the end of the 5th month the pulse had come down to 72 and continued steadily at this rate, and though still more or less Rheumatic he was able to resume his duties in full and by the use of a stove in his office and the total avoidance of Beer and all fermented liquors he kept moderately well for a period of 3 months.

Feb. 28th, 1876. For a week previous the patient had been ailing, bilious and out of sorts with marked acidity of the breath and perspiration, pain and swelling in the smaller joints. Then a febrile condition set in with increased pain, lameness and swelling of the wrists and finger joints: tongue furred, temperature 100 degrees, pulse 120. I was pleased to find an improvement set in within 6 days, from the use of Fowler's Arsenical Solution with alkalies and Quinine, which

however was interrupted by a Bronchial attack with Bronchorrhœa. A relapse took place at the end of a fortnight when he had carbolic acid baths in addition, from which he also derived benefit, but dreading a repetition of the same wearisome round of improvements and relapses, as he went through last year, I clutched at the promise held out by a new remedy, and commenced the administration of Salicin in 15 grain doses twice a day. This was on the 18th of March, the 3rd week of the present attack. The improvement was sure, steady, and abiding. On the 20th he is reported as keeping steadily better, pulse down to 90, temperature natural, sleeping well, appetite excellent and in no pain though some tumefaction of the joints still continues.

On the 6th of April while in his office he caught cold and got a Neuralgic attack with excited pulse which disappeared as quickly as it came under a dose or two of Salicin with Iodide of Potassium. He has continued to enjoy the best of health ever since, taking an occasional dose of the Iodide, and having at my special request given up the use of all fermented beverages.

This case is the first recorded (so far as I am aware) in which a clear connection would seem to have existed between Lead-impregnation and susceptibility to Rheumatic fever and Rheumatism. His general habits, the use of beer as a beverage, his excellent assimilative powers, deficient exercise and tendency to obesity, were such as in a subject of Lead-impregnation, should rather have led to Gouty attacks, yet the symptoms were those of acute or sub-acute rheumatism with special peculiarities. The joints attacked were remarkable for an œdematous puffiness of the cellular tissue and a superficial bluish or purplish hue, unattended by effusion into the joints, while the remarkably irritable state of the heart and arteries, unaffected by the most powerful nervine sedatives, pointed to the existence of a virus in the blood, which had first to be eliminated before the Anti-rheumatic remedies could take proper effect. As the nervous element so largely predominated amid the general features of the case, it is easy to understand the advantage derived from the exhibition of Quinine, Arsenic, and finally Salicin which gave the disease its coup-de-grace.

The Sources of Lead-poisoning are almost as various as the uses to which this useful but dangerous metal is applied. It is not merely Plumbers, Painters, and Japanners, who are liable to be poisoned by testing or inhaling the metal but those who drink Cider, Perry, and Gingerbeer; since these beverages, it appears

are fermented in badlyglazed vessels called in Hampshire "bushel-pans" the glazing of which is produced by fusing a mixture of litharge and barley-meal, and which is soluble in Tartaric and other vegetable acids. A whole neighbourhood was found in one instance, to have become victims to Lead-colic through the care-for-nobody policy of the miller, who filled up the deficiencies in his millstones with lumps of lead! Black silk thread is said to be steeped in a solution of acetate of lead and exposed to a current of Sulphuretted Hydrogen—the first to give weight, and the last colour: glazed visiting cards and wax dolls are dangerous play-things for children from the presence of Carbonate of lead: acetate of lead when administered medicinally for homorrhage is fraught with danger in some cases, and above all leaden cisterns must sometimes be a source of lead-impregnation, more especially when new or recently cleaned out, and at joinings where the opposition of an iron pipe sets up galvanic action and oxidation of the lead!

So far as we know at present the results of lead-impregnation may be summed up under the following heads:—

1. There is generally a period in which the patient simply feels unwell without any more marked symptoms than foul breath, nausea, muscular languor and occasional griping pains in the abdomen.

2. There supervene upon this secondly, attacks of intense, abdominal twisting pain, chiefly around the navel, but not always confined to the region of the transverse colon and sometimes attacking the stomach simply, with more or less complete obstruction of the bowels, slow depressed pulse and general prostration. In a word Painter's colic with the confirmatory sign of a bluish-purple line along the gums and encroaching on the teeth. This is attributed with probable correctness to the presence of Sulphide of Lead: the evolution of Sulphuretted Hydrogen from the Tartar on the teeth yielding the necessary conditions. Depression of the animal spirits is a constant accompaniment of Colica Pictonum and there is a marked diminution of the renal secretion and of the amount of urea daily eliminated. For these conditions half-a-

drachm of Battley's sedative with half-a-drachm of Sulphuric læther in Camphor mixture followed up by an ounce of the Sulphate of Magnesia is the most efficacious treatment. A hot bath, a warm water enema, and the constant current, may also be had recourse to, but the former measures alone will in most instances be sufficient to effect a cure, more especially if the Iodide of Potassium be administered for some time afterwards. Castor oil is a favourite remedy at some Lead-factories and may alleviate by its combined aperient action and solvent power over lead—lead being miscible in oil: and the bath of Sulphide of Potassium (4 ounces to 30 gallons) effectually relieves even acute cases. The skin becomes blackened from the formation of Sulphide of Lead (Pereira), (De Mussy)—the skin being by far the most important emunctory of Lead (Begbie.) M. Gendrin affirms that sulphuric acid acts both as a prophylactic, when taken by the workmen as Sulphuric acid Lemonade, and as a remedial agent in cases of Lead Colic, curing in from 3 to 7 days, but that it fails in cases of Chronic Lead palsy. The plan above suggested is much more speedily curative. Bennet of the white Lead works, Birmingham, equally affirms the prophylactic virtues of the Sulphuric acid Lemonade, while the observations of Tanquerelle and Grisolle are directly opposed to such conclusions. In Germany the alum treatment is said to be very successful.

3. There is thirdly a series of Neuropathic lesions taking the form of Rheumatic pains, Neuralgia, Pleurodynia, Sciatica, Asthma, Pleurisy, Eclampsia, Insanity, Paralysis, which are plainly traceable as more chronic results of lead-impregnation and which are best treated by the continuous administration of the Iodide of Potassium or Sodium with Sulphur-baths, and Codliver oil. Calomel (whether by stimulating the absorbent and eliminative glands) wrought a cure in one of the worst of these cases.

4. It is believed that in many, if not in all cases presence of Lead leads to an arrestment, more or less pronounced of the Secretary function of the Kidney, hence the urine is often pale, I efective in erinary salts and of very low specific gravit. The

Eclampsia may therefore be regarded as due to uræmis poisoning and in one case at least was accompanied by albuminaria.

5. Lead-impregnation in connection with the use of fermented liquors such as beer and porter has been shown by Dr. Garrod to be a powerfully predisposing cause of Gout.

6. Lead-impregnation in other cases probably connected with the use of ardent spirits such as whisky, brandy, &c., predisposes to a form of Rheumatism—irritative, subacute, persistent. The treatment which has been found most successful is a combination or alternation of the Iodide of Potassium with Salicin. The latter would seem to have the power of checking the formation of lactic acid.

7. There is a plumbo-cachectic appearance or Lead-cachexia, in the subjects of Lead-impregnation, which is quite characteristic. It is compounded of anæmia and pallor, with a waxen bluish-white or yellowish-white complexion and may be taken as truly indicative of Lead-poisoning even in the absence of the gingival blue line. How is this explained? Lead has a greater affinity for animal matter, as well as a stronger tendency to combine with organic acids than perhaps any other metal has. Witness the way in which the various animal juices, colouring matters, and organic matters are so readily thrown down by subacetate of lead! Thus it is no doubt that the blood deterioration is induced, till at last the true saturnine cachexia is developed, the metal being deposited in the molecular state or in the form of suboxide in the various solid tissues as brain, muscle, liver, skin. When redissolved as by the action of the Iodides it is eliminated by the intestinal canal, the kidneys and the skin. Hence Sulphureous baths will cause the surface of the body to be discoloured in the most curious manner. Clapton & De Mussy, saw the skins of patients suffering from Plumbism decidedly blackened in certain parts while in these baths.

8. More knowledge is wanted of the Chemistry of the living fluids, but the evidence of the solvent and eliminating power of the Iodides of Potassium and Sodium over Salts of lead, Mercury and Copper may be considered as fully established. A case has been

related where Enterodynia was induced, in an old long-quiet case of Plumbism with wrist-drop, from the solvent action of the Iodide bringing the lead afresh into active force within the system, and the last of my cases affords another illustration of this contingency. It is doubtless eliminated as a double Iodide, i.e., of Lead and Potassium.

The great object in treatment is to get the poison out of the system. Nervine tonics, galvanism, chalybeates and blood restoratives are all useless until elimination is effected. Iodides, the purgative Sulphates and Sulphur-baths are the most reliable. In general the Sulphates are best adapted for acute cases, the Iodides for chronic ones, but an alternation is commonly desirable.

TURNING.

NOTES OF FORTY CASES WITH REMARKS.

By F. H. V. GROSEHOLZ, L.K. & Q.C.P., Irel., &c.

(Read before the Manchester Medical Society, February, 1877.)

MR. PRESIDENT AND GENTLEMEN,

It is not my intention to give a detailed account of each individual case of the forty in which I have thought it necessary to perform the operation of Version; as, though such minuteness might prove not altogether uninteresting, nor entirely unprofitable, yet, doubtless, a summary of the facts and the deductions to be drawn therefrom will more graphically and at less expense of time effect the object I have in view, viz:—The consideration of the causes which demand an artificial termination of labour by the aid of Turning, the results of that operation as regards both mother and child; the method and time of performing this operation; and the lessons which are taught by its results whether successful or otherwise.

First, then, as to the *Causes which necessitated Version*.—As “turning” is generally understood to mean a restoration of a child to its normal position in the uterus or the substitution of some other portion of the body for that originally presenting, it will not seem surprising that the operation was performed for the rectification of some mal-position of the fœtus in 32 out of the 40 cases. Of the remaining eight, 3 were cases of Placenta Prævia, 3 of contraction of the Pelvis, and 2 of Uterine Inertia.

We thus see that 32, or four fifths of the cases in which the operation was performed, were cases of *mal-presentation* of the foetus, 3 of *mal-formation* of the mother, 3 of *mal-position* of the Placenta; and 2 of failure of uterine contractions. To go a little more into detail I have tabulated the above facts as follows:—

MALPRESENTATION—	{	Shoulder	17	}	32
		Arm and Funis	7		
		Arm	3		
		Face and Hand	3		
		Brow	1		
		Back	1		
PELVIC CONTRACTION		3			
PLACENTA PRAEVIA		3			
UTERINE INERTIA		2			
			<hr/> 40 <hr/>		

Secondly, as to the *Results* of the operation as regards both mother and child. The average maternal mortality has been estimated by Dr. Churchill as 1 in 15; and the infantile as 1 in $2\frac{1}{2}$.—In my forty cases I have had no deaths among the mothers; eighteen children were either born dead, or died soon after birth.

This seems a rather high infantile mortality; it is indeed in the proportion of 1 death in $2\frac{1}{2}$; but as 7 of the children had manifestly been dead for some time, (as evinced by signs of putrefaction more or less advanced) it is hardly fair to class theirs among the deaths due to Turning.

Omitting these, the proportion of dead children delivered by Version is exactly 1 in 3. Of the other 11 children which were born dead, 2 were cases in which the funis was prolapsed; 1 in which there was Placenta Prævia; and the remaining eight might be roughly classed together as owing their death to a disproportion between the maternal pelvis and the infantile cranium.

One woman was only twenty, another as much as forty-five years of age.

One child weighed 13lbs and 2 oz.; and one, which lived nearly two days, weighed only 2lbs. 1 oz.

Having enumerated the causes which demanded Version, I will next briefly remark on the signs by which I have been guided in my selection of the time which seemed most favourable for performing the operation.

In every labour, all obstetricians will concede that it is of the utmost importance to become acquainted with the exact position of the child at the earliest possible opportunity.

Where there is a mal-presentation its early discovery is of the greatest importance; for before the membranes have ruptured not only can turning be performed more easily and with less risk to the mother, but the chances of the child's life are also increased.

After the membranes have ruptured, the danger to both mother and child becomes greater and greater as time goes on. Indeed no cases require such gentleness, patience, and courage on the part of the operator, as those cases of arm presentation in which the membranes have long been ruptured, and the uterus is closely contracted around the foetus, pressing it more and more firmly down into the Pelvis.

In cases where the Os has not been sufficiently dilated to admit the passage of the hand, and where a speedy termination of labour was essential, I have found very satisfactory results from the practice of gently and gradually dilating the os by passing the thumb and first two fingers within it; and have, by their aid, without the use of anything like undue force, frequently been able to effect a comparatively speedy dilatation. In one case I used Barnes' Dilators, and they most fully answered my expectations.

I need not say that Braxton Hicks' method of Version by combined internal and external manipulation, not only greatly facilitates the operation, but also enables us to perform it sooner than would be justifiable were it necessary to introduce the whole hand within the Uterus. Indeed if called to a case of arm-presentation in which the membranes are unruptured, and the Os though small, yet soft and dilatable, I see no reason to defer the operation; for, in awaiting the fuller dilatation of the Os, we run the risk of a sudden rupture of the membranes, thereby losing the chance of an easy, safe and successful termination of the case.

Having now cursorily considered the *period* at which it is best to perform Version, I will proceed to describe the *manner* in which I usually perform the operation. The ordinary obstetric position, (the patient being on the left side,) is, I think, preferable to any other, though in a few very difficult cases, where the abdomen of the child was placed anteriorly, and where the liquor anmii had long escaped, I have found it advantageous for the mother to lie on her back, and have thus been more easily able to pass the hand over the child's body.

In nearly every instance I use the left hand, as I think it much more readily adapts itself to the curve of the sacrum, leaving at the same time the right hand at liberty to manipulate externally; then, too, the left hand is generally smaller than the right,—a not unimportant point to be considered in the choice of the operating hand.

It is not necessary for me to describe in detail the operation of Version, for with that you are well acquainted, whether it be the simpler and safer plan of turning by combined internal and external manipulation, or the more serious and difficult operation, when it is necessary to introduce the hand into the uterus from which the waters have escaped, and which is firmly contracted around the child. On one or two points, however, I should like to say a few words.

As long as the operator's hand can take hold of one of the inferior extremities, it is immaterial whether a knee or a foot be seized. In transverse presentations traction on the knee or foot of the side opposite to the presenting shoulder effects rotation more readily than by bringing down the leg of the same side. It is decidedly advantageous to bring down only one foot or knee instead of both, as in this manner the parts are better dilated, and less difficulty is experienced in the delivering of the rest of the body. Where, after bringing one foot down, the body does not readily turn, it is a good plan to pass a tape round the limb, and whilst making traction on it downwards and backwards, to attempt to push back the presenting shoulder.

In Placenta Prævia I prefer to turn by Dr. Hicks' method as soon as the condition is recognised; where it is necessary to pass the hand into the uterus, I try to do so at the point where the Placenta seems least attached, and never through its substance.

Knowing the extreme value of Chloroform to cause relaxation of the spasmodically contracted uterine fibres, it will doubtless seem strange that I have not used it in a single case; this, I can assure you, has been *malgré moi*; for, unfortunately my patients and their friends have all shown such a determined opposition to the administration of Chloroform, that I have on every occasion been compelled reluctantly to proceed without the aid of anesthesia.

That this has greatly added to the risk of laceration of the uterus there can be no doubt, nor that it has also added enormously to the anxiety, difficulty, and pain experienced by the operator, and, under the circumstances, I think it remarkably lucky that though there have been several cases of tedious convalescence, yet all my forty patients have eventually completely recovered.

I have not entered into the controversy as to the relative merits of Version, Long Forceps, and Craniotomy, as I fear I have no fresh facts to produce that would throw much light on the subject. There is no doubt that each method has peculiar advantages in certain circumstances, and that every accoucheur should be intimately acquainted with *each* obstetric operation; yet at the same time none requires and deserves more careful study and practical skill than the ancient, most important, and truly conservative operation of Turning.

ON CHRONIC ATROPHIC RHINITIS.

By J. DIXON MANN,

Licentiate of the King & Queen's College of Physicians, Ireland.

THE mucous membrane lining the nasal cavities is liable to two forms of chronic inflammation; in the one the membrane is swollen, its vessels are enlarged and its secretion is depraved and greatly increased in quantity; in the other the membrane is thinner than normal and is devoid of secretion; it is to this, the atrophic form, that the following remarks apply.

Chronic atrophic rhinitis is usually a sequence of hyperplastic rhinitis; exceptions however being occasionally met with where the disease exists *per se*. Chronic rhinitis is usually initiated by an attack of nasal catarrh which instead of subsiding in the usual manner passes through an additional train of symptoms. The muco-purulent secretion, at first excessive and discharged continuously, after a time diminishes in quantity and comes away at intervals in viscid lumps of bright yellow matter. When examined microscopically, this matter is found to consist chiefly of epithelial debris, pus corpuscles, large granular cells, and free fat granules; to the latter it owes the brightness of its colour. If the disease merges into the atrophic form, the secretion goes on slowly

diminishing in quantity, becoming more tough and difficult to get rid of; it is usually detached with considerable effort first thing in the morning. After a still longer interval, the discharge is retained until it is thoroughly inspissated, it then comes away in the form of scabs of a green, or greenish brown colour; sometimes the scabs are tinged with fresh blood, the result of premature detachment from the mucous membrane. The disease has now arrived at the atrophic condition and will continue, with fluctuations, in this state for months or even years, unless cut short by treatment.

The patient during the earlier stages complains merely of the inconvenience caused by the constant discharge from the nose, and has little or no pain. When the atrophic condition is reached and has continued some time, new symptoms manifest themselves. The patient now complains of a peculiar feeling of constriction at the roof of the nares, he becomes moody and despondent, his thoughts are focussed on his ailment and his conversation invariably drifts to the subject of his sufferings. He anxiously examines whatever is discharged from the nostrils and carefully preserves it for the inspection of his medical attendant. He dislikes the effort of speaking, especially in public, or to strangers; the voice is changed although the nasal passages are quite patent; the mental powers are enfeebled and the patient manifests a reluctance to meet any but his immediate relations; he professes himself unable to co-ordinate his ideas and shrinks from any mental effort. The above description may seem exaggerated, there being such an apparent discrepancy between cause and effect; I have, however, seen more than one case in which the friends of the patient were apprehensive of softening of the brain. The mental symptoms, as might be expected, display themselves most markedly in educated and sensitive persons. The close proximity of the mischief to the brain doubtless has a depressing influence, if not directly at least indirectly, in causing the patient to imagine that the brain itself is the seat of the disease.

On making a rhinoscopic examination, a portion of the mucous membrane of the posterior nares will be found dry and devoid of

epithelial covering. The roof of the pharynx, or the posterior portion of the velum are usually the parts affected; in some cases the membrane covering the septum and the anterior portion of the nasal passages is affected, and may be seen from the anterior nares if the nostrils are dilated with a Fraenkel's speculum.

On some portion of the diseased membrane a scab of inspissated muco-pus, which has been secreted above the seat of mischief, will probably be seen, and in prolonged cases abrasions or even small ulcers extending partially or wholly through the basement membrane. Muco-pus adheres to the denuded membrane with great tenacity; when deposited on healthy membrane it is soon washed off by the normal secretion; but on the dry membrane muco-pus adheres until it is thoroughly desiccated. The contraction produced by the desiccation causes the edges of the scab gradually to curl up from the surface on which it lodges and so in time liberates it. It is requisite to caution the patient against making too violent efforts to detach the offending fragment, for if a powerful blast of air impinges on the everted edges of the scab and tears it off before it is sufficiently loosened, a portion of the basement membrane is torn off with it, and an abrasion or an ulcer is the result.

The symptoms of atrophic rhinitis it will be seen are mostly subjective, and unless the disease is traced from its initial phase it is liable to be overlooked; this is especially the case when the patient neglects to place himself under treatment until the primary symptoms have subsided, the rhinoscope then affords the only means of arriving at a correct diagnosis.

The treatment of atrophic rhinitis, so far as my experience is concerned, was for a long time, very unsatisfactory; astringent and stimulating douches, the inhalation of atomized fluids, medicated powders snuffed up the nostrils with various kinds of general treatment, all failed, in the majority of cases, to afford relief. The treatment I have found the most successful, suggested itself to me through observing the almost magical effect produced by dilute red oxide of mercury ointment upon the dry state of the

lips caused by colds, and also upon the abrasions left by herpes labialis. This preparation of mercury has a very stimulating effect upon abraded mucous membrane, causing rapid proliferation of the epithelial cells. To apply the ointment, a small camel hair brush is carefully fastened to the end of a straight piece of "hard drawn" copper wire such as is used for bell hanging, about seven inches long; the end to which the brush is attached is then bent at an angle to the shank in accordance with the position of the part to be treated. If it is the roof of the pharynx the angle should be about 100 degrees; if the posterior part of the velum 70 or 80 degrees. The shank of the wire is fixed in the handle of a laryngoscopic mirror and the brush well coated with an ointment of equal parts of red oxide of mercury ointment and lard; (in some cases the ointment may be used undiluted.) The brush is then introduced into the mouth more or less sidewise, slipped under the soft palate, then turned upwards and directed to the part affected and gently moved to and fro for a few seconds. The ointment readily adheres to the dry membrane and causes little or no pain; two or three applications may be made at each sitting, every day when practicable, or at least three times a week. To touch the posterior part of the pharynx the brush handle should be elevated; to touch the back of the velum it should be depressed. Some little dexterity is required to pass the brush *cleanly* into position; if it comes in contact with the velum during introduction, muscular contraction is induced which closes the communication between mouth and nose. If the disease is situated in the anterior nasal passages it is better to coat the bulb of a small olivary bougie with the ointment and pass it up the nostril, taking care not to touch the walls until the seat of mischief is reached. The black French bougies are the best for this purpose as they are much more flexible than the brown bougies.

The general treatment must be in accordance with the special features of each case. In strumous subjects it will comprise cod-liver oil, quinine and iron; in syphilitic cases iodide of potassium should be administered; chlorate of potash in ten grain doses

twice or thrice a day is very valuable; when practicable sea air should be recommended.

CASE 1. A gentleman past the prime of life but vigorous and healthy, with the exception of having had several severe attacks of epistaxis previously to coming under my care. During one bad attack in the spring of 1873, he was plugged both anteriorly and posteriorly, the plugs being allowed to remain until the third day; considerable pain was experienced during and after the removal of the plugs. In a few days a thick purulent discharge tinged with blood appeared which continued for three or four months. On May 4th, 1874, the epistaxis returned on which occasion I first saw the patient; the bleeding was not severe and was readily controlled by the subcutaneous injection of ergotine. Since the commencement of the purulent discharge from the nares, the patient had by degrees become extremely hypochondriacal, so much so that his friends feared mental alienation. He was a man of great mental attainments but now had quite forsaken his intellectual pursuits and devoted the whole of his attention to his ailment. He complained of a peculiar pain at the base of the brain, and of a faint "burnt smell" which varied in intensity but did not interfere with his olfactory powers. As an example of his perturbed state of mind I may mention that one day he shewed me a small black substance expelled from the nares which he feared was necrosed bone; on making a section of the substance and examining it with the microscope, I found it was a fragment of a coffee berry that had made its way into the nares from the breakfast table. On examining the posterior nares with the rhinoscope, the mucous membrane covering the middle and lower turbinate bones (on their posterior aspect) and a portion of the velum was seen to be dry and glazed; a scab covered a portion of the dry membrane. A little time afterwards I had the good fortune to examine the patient immediately after the scab was detached and found in its place an abrasion about the size of a split pea from which bloody serum was exuding. The patient was treated locally with the mercury ointment and the mucous membrane gradually recovered its healthy appearance. The following spring he had another attack of epistaxis which was checked without plugging; this was followed by a slight return of the epithelial denudation which again yielded to treatment. In March, 1876, a very slight bleeding occurred followed by a renewal of the old symptoms, on this occasion confined to the anterior passages; local treatment was again resorted to with complete success. The patient is now quite well, is in good spirits and has regained his pristine mental vigor.

CASE 2.—A medical man, about 30 years of age, consulted me in the early part of last year concerning an affection of the posterior nares which he feared was the secondary result of syphilitic inoculation received in the course of his professional duties from a parturient patient. He vividly described the trouble

and annoyance he underwent every morning in dislodging a mass of inspissated mucous, and said that until he succeeded he was quite unable to attend to his duties. He feared disease of the hard palate and was consequently much dispirited; he had undergone general syphilitic treatment. Rhinoscopic examination revealed a glazed condition of the mucous membrane covering the greater part of the posterior nares, with patches of inspissated mucous adherent here and there; there was no abrasion. I made the usual application to the nares, and explained to the patient how to continue the treatment; unfortunately he resided in the country some distance away and could not see me as often as required; the last time I saw him he was much better, but from want of regular treatment had not improved as he otherwise would have done.

CASE 3.—*æt.* 46. This patient was sent to me July 17th, 1876, by my friend, Dr. Little, under whose care he had previously been for syphilitic iritis. The patient graphically described the symptoms of Atrophic Rhinitis; he suffered from the characteristic local pain, had lost his aptitude for business, and was troubled with accumulations of thick matter especially when talking—to use his own words, his “greatest pleasure was to sit in a dark corner away from every one.” On inspecting the mouth, I found the tonsils and uvula so congested and irritable that a rhinoscopic examination was impossible. The fauces were therefore swabbed out with a strong solution of nitrate of silver every other day for a week when the irritation had disappeared. The rhinoscope shewed in addition to the dry membrane, a small ulcer on the velum extending through the basement membrane; it had the characteristic appearance of a syphilitic ulcer.

By means of a bent wire tipped with nitrate of silver, I touched the ulcer at intervals three or four times; to the rest of the membrane mercury ointment was applied. The patient was taking iodide of potassium when he came to me which was continued. The local treatment was diligently persevered in for upwards of two months, and for some time longer the patient occasionally presented himself for an application. The local symptoms have now entirely disappeared and the patient can converse without difficulty; his mind has also recovered its healthy tone.

CASE 4.—A lady *æt.* 48, unmarried, enjoying good health, took several severe influenza colds in rapid sequence in the winter of 1873. When I first saw her she was suffering from chronic nasal catarrh with a profuse purulent discharge free from smell. I tried a variety of remedies local and general, including iron, quinine and copaiba, with douches of tannin, alum, permanganate of potash, &c., with little effect. Eventually, early in the spring, she went to the south of England and remained there six weeks. On her return I again saw her; the discharge had ceased but still she said that “all was not right.” She felt a pain along the

nasal passages "as though they had been scalded" which was aggravated by breathing cold, dry air, and which was temporarily relieved by the inhalation of aqueous vapour. On examining with the rhinoscope, the mucous membrane covering the posterior aspect of the turbinate bones presented a marked contrast to the healthy appearance, being dry and devoid of its usual mucous coating; the velum and pharynx were healthy. The general health being good, local treatment with the mercury ointment alone was resorted to. The improvement was more rapid than in the previous cases, the epithelial covering being renewed in about three weeks.

These cases are selected from many others as typical of atrophic rhinitis and its varied causation. Case 1 was the sequence of traumatic rhinitis; cases 2 and 3 were of syphilitic origin, and case 4 followed an attack of simple nasal catarrh.

GLEANING'S FROM OBSTETRIC CASES.

By J. ARMSTRONG, M.B.,

Senior Assistant Physician, Infirmary for Children, Liverpool.

I. A CASE OF VAGINAL HÆMATOCELE :—

A Midwife sent for me to see this case on finding a something in the vagina after the placenta and membranes had been expelled.

On Vaginal examination I felt a tumour in the posterior vaginal wall extending to within one inch of the labia and distending the Vagina. It felt like the foetal side of a placenta and was bluish in colour.

The mother told me that directly after the expulsion of the child she was seized with an intense pain in the seat of the tumour.

Diagnosing the tumour to be an Hæmatocele, I enjoined perfect rest and cleanliness, and prescribed an opiate to relieve the pain.

On the fourth day, the tumour burst. Lumps of coagulum and bits of mucous membrane came away. The discharge now became very offensive.

Quinine in 3 grain doses was administered and vaginal disinfectant injections used. After being three weeks in bed, the patient had quite recovered and no trace of the tumour could be felt.

Vaginal Hæmatocele is of rare occurrence ; though generally in connection with the pregnant state, yet cases occasionally arise in the non-pregnant.

The previous history gave no clue as to the cause. The mother was 35 years of age, was healthy, and had no varicose

veins. No accident befell her during pregnancy. She had a child 8 years previously.

The pathological condition appeared to be, blood powered out beneath the mucous membrane, from a ruptured vein or plexus of veins. Pregnancy giving rise to congestion of the pelvic veins, predisposed to this condition. The exciting cause seemed to be violent uterine contractions, intensified by the mother being on her knees on the floor at the time of delivery; this position giving her every facility for bearing down. The abuse of Ergot by increasing uterine action, would also act as an exciting cause. To lessen the risk of such a pathological condition arising, the bowels should be kept regular during pregnancy to relieve uterine congestion, and during the 2nd stage of labour the mother should be kept in bed. *ref*

Should the tumour be diagnosed whilst forming, ice applied locally, might stem the flow. When once formed there can be little doubts as to the propriety of leaving it alone for 12 or 24 hours; unless urgent symptoms indicating loss of blood should arise. After this period the question arises, should the tumour be incised or left to nature.

Judging from the successful termination of this case I should not hesitate in a similar case to leave it alone.

There was a very fetid discharge after the tumour burst and if we could be sure that by incising it after 12 or 24 hours, this condition could be prevented, then there is some reason for doing so. The lochial discharge being very liable to take on unhealthy action it is well to have as few absorbing surfaces as possible. If nature is allowed to evacuate, the parts involved are more likely to be in non-absorbent condition, and whilst the mucous membrane is intact the coagulum is harmless.

Should nature fail either to evacuate the contents of the tumour or absorb them, incision should be had recourse to.

II. A CASE OF PUERPERAL SEPTICÆMIA.

I attended a primipara, aged 25, in a natural labour, previous to which her

health was good. For a day or two the lochial discharge was profuse. On the fourth day, it became scanty and fetid. She was feverish, and diarrhoea supervened with sickness. This state continued, sometimes more, sometimes less, yet in spite of this she ventured downstairs on the ninth day, on which day she had a rigor. From this period she suffered from the following symptoms. The diarrhoea and sickness continued, her tongue was glazed and red, uterine pain was complained of, the lochial discharge and secretion of milk were suppressed. On the third day, after the rigor, a swelling appeared in the left submaxillary region which resulted in an abscess and burst into the ear. At this period the fauces were coated with a white deposit resembling in appearance diphtheritic exudation. Pain over region of the kidneys was complained of, and the urine was scanty and albuminous. The pulse varied from 140 to 160, and was soft and thready. The temperature ranged from 103 to 106.

These symptoms lasted about nine days when pulmonary symptoms set in. There was dulness posteriorly over the bases of both lungs, and râles were heard over the chest. The cough was troublesome and the expectoration mucopurulent.

At this stage the face was anxious, sunken, and sallow, the breath was sweet as in pyæmia and there was a low muttering delirium.

About the fourth week after the rigor, she began to improve. The convalescence was steady.

The treatment consisted chiefly of quinine, brandy, cold meat, soup and milk. The nurse used vaginal disinfectant injections regularly.

From the above facts I draw the following conclusions :—

1. That this was a case of Puerperal Septicæmia probably due to self infection, its origin being a foreign body retained in the uterus, which decomposed and was absorbed. The foreign body may be a bit of placenta, or membrane, or retained coagula. I had no reason to think that it could be any one of these. The origin might also be external to the mother, the obstetrician may be the medium of infection, or the nurse may, or again the lochia might take on unhealthy action from endemic causes, such as defective drainage, and be absorbed.

Whatever the origin of the Septicæmia was, its products were a scanty and fetid then a suppressed lochial discharge, suppressed secretion of milk, uterine phlebitis, diarrhoea and sickness, a submaxillary abscess, pseudo-diphtheria, pneumonia and nephritis.

2. That Puerperal Septicæmia due to self,—infection is not infectious, that is through the atmosphere. Whilst in attendance upon such a case the obstetrician is justified I think in attending other midwifery cases, provided that his hands do not come in contact with the lochial discharge of the infected patient, or if they do, that he ensures perfect disinfection. I think that a case of Septicæmia from self,—infection might be treated in the Lying-in-Hospital with impunity to other patients, with the proviso that the nurse attends only to this case. I don't allude here to the infectious forms of Puerperal fever such as the Scarlatinal.

3. That the symptoms resembling diphtheria did not justify the term puerperal diphtheria. At first I thought that this might turn out to be a case of Diphtheria, but the fact that the white coating on the mucous membrane of the fauces and cheeks could be rubbed off without causing a breach of the surface, led me to think otherwise.

Might the coating not be a deposit of the nature of sordes as seen in the Typhoid state ?

4. That with regard to treatment, Quinine in 3-5 grain doses, given simply in cold water and repeated every four hours was most useful in sustaining the strength, and in reducing the temperature. In all cases of Septicæmia it is most desirable to find out if there is a foreign body in the uterus so that it may be removed without delay. Vaginal or better still uterine disinfectant injections must be constantly used. The difficulty is, that should the obstetrician administer the uterine injections he cannot without great risk attend other cases of labour, and should the nurse do so the vagina only, if that, will be injected. The same difficulty exists in making vaginal examinations. In this case I refrained from doing so, for the following reasons though believing this to be generally speaking, our duty, the labour was a natural one, the placenta and membranes entire were expelled and the uterus was firmly compressed to expel any coagula that might be retained.

It is the duty of the obstetrician so as to guard against being the medium of infection most scrupulously to disinfect his hands,

and certainly always to change his coat when in attendance on other cases.

III. A CASE OF PUERPERAL ECLAMPSIA, AT THE 7th MONTH.

I saw this case for the first time four hours previous to the first seizure. She then complained of sickness. She was a primipara, 27 years of age, and enjoyed good health. About 10 or 12 days before I saw her, she noticed that her ankles began to swell. There was puffiness of the lower eyelids, and the palms of her hands and the soles of her feet were inflamed and tender.

At the onset of the convulsion I was again sent for. When I arrived she had had two fits. There was an interval of a quarter of an hour between the fits.

From this time the convulsions were controlled by means of chloroform.

Dr. Grimsdale saw this case with me and agreed that an Anesthetic to subdue the convulsions and to bring about labour, were the two indications.

As the convulsions could be kept under by chloroform and as the os was not dilatatable time was allowed for the parts to relax.

After due time and finding that the fits still continued when the effects of the chloroform had passed off, dilatation by means of Barne's Bags was commenced with. Whilst this was being done, Dr. Rawdon prevented the convulsions with chloroform.

Before dilatation was begun, the little finger only could be inserted into os. The head presented and the liquor amnii had not escaped.

The 1st and 2nd sizes of Barne's Bag's answered the purpose admirably but on account of the contents of the uterus the largest size was not retained. This size was not persevered with so as not to rupture the bag of membranes.

Three fingers could now be inserted into the os, allowing the Braxton Hicks' method of turning to be readily accomplished. The knee was seized and the bag of membranes ruptured. Now would have been the time to use the largest size of Barnes' Bag's, but I did not think about it; just then about every 10 or 15 minutes assistance was given and the child's body was gradually extracted.

Every dodge was tried to get the head away. At last the short forceps was had recourse to.

The mother having been more or less under the influence of chloroform for 10 hours, the placenta was peeled off as there was not sufficient action in the uterus to expel it. The uterus contracted after this and to ensure its continuence Dr. Rawdon suggested that an injection of Ergot be administered.

As the child had been dead two or three days, the difficulty in extraction was greatly increased.

Instructions were given to the nurse to give one teaspoonful of Brandy every hour, beef tea and thin gruel in the intervals, and to keep the patient's body warm.

She remained unconscious for 48 hours—but could swallow what was ordered. A dose of castor oil and plenty of warm ailments were administered to arouse into action the bowels and skin.

On the 3rd day, she began to rally from the unconscious state. She complained of great soreness about the uterus. She made a good recovery. At the end of a fortnight, she could sit up and in three weeks could walk about.

In offering remarks on this case I may say that if the line of treatment now stated be early carried out the prognosis is generally favourable. No one can doubt the gravity of such a case, or the fearful responsibility that rests upon the obstetrician when called to such a case.

The line of treatment is clear; the difficulty is in carrying it out. When the convulsions supervene, labour should be brought about with the least possible delay. It is necessary to subdue the fits, and I think that chloroform is the most manageable of anæsthetics.

In a case like this, when chloroform was most effectual in staying the fits, bleeding would have been quite uncalled for.

If there is one thing more important than another to attend to, it is to imitate the natural process—allowing an interval between the attempts at dilatation and extraction, not only gives the parts time to relax, but it recuperates the strength both of the patient and obstetrician.

It is very necessary also to preserve the bag of membranes intact, otherwise the bi-polar method of turning could not be accomplished.

When we consider the cause of the convulsions as being mechanical, it is very doubtful if we can by any mode of treatment prevent them.

When once established the duty of the obstetrician is to remove the cause without delay, knowing that the longer the delay the greater danger.

If the cause of the Eclampsia were purely mechanical one would be led to think that, by drawing off the liquor amnii to the amount not prejudicial to the child in utero, would lessen the liability to the attacks, but I doubt if this would be practicable.

CASES FROM SURGICAL PRACTICE.

TWO CASES OF OVARIOTOMY WITH REMARKS.

By **ANDREW BONTFLOWER,**

Honorary Surgeon to the Salford Hospital.

It has become a regular custom for medical men to publish their cases of ovariectomy whether successful or the reverse—and it is no doubt in a great measure owing to this fact, that the percentage of deaths after this operation decreases year by year. Little by little have doubtful and disputable points been cleared up,—and the result is that the greater details attending the operation have been tolerably fully elaborated—without being over sanguine, from future records by carefully sifting out, and clearing up the questionable points which more concern the minor details, before and after the operation, we may perhaps some of us see the day, when ovariectomy may be looked upon as no more dangerous to life than an amputation of the leg or thigh.

Certain facts must be cleared up in reference to the adhesions of the cyst to its surroundings. Why so many cases, which at the time of operation appear most favourable without any adhesions frequently terminate in death, and other cases most which appear unfavourable on account of the existence of adhesions, so often

recover. Whether the explanation lies in the fact, that in cases with no adhesions, the peritoneum retains all its habitual delicacy and death is caused by peritonitis, and that where adhesions are numerous, this membrane becomes thickened, more or less altered in character, and less liable to inflammation, and no peritonitis results—or whether the difference in results lie more in the fact that in the one sufficient regard is not paid to perfect cleanliness, and in the other some form of antiseptic treatment is carefully followed out. Other questions remain to be settled. Which is the best clamp? What preparation, if any, is to be made beforehand in dieting, and otherwise preparing the patient? What the increase of risk if the pedicle be ligatured and returned into the abdominal cavity? What is the best mode of treatment after operation? Probably the result in many cases of ovariectomy turns on one or other of these points.

One recent writer in the '*British Medical Journal*,' one who has had a large and successful experience in ovarian operations, says he always makes it a rule to bleed to the extent of several ounces, before operating, and also for some days previously, deprives the patient of flesh meat. Whether this severe plan is advisable or not, or in any way turns the balance in doubtful cases remains to be solved, yet all will agree that careful and judicious diet, perfect rest, and prohibition of any examination of the tumour for a few days before the operation will in all cases be beneficial, and assist in some degree, the case to a favourable issue.

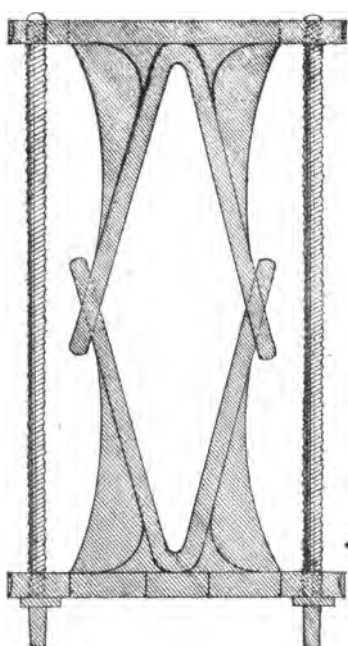
It has been urged again by some, that as the mortality after ovariectomy is so great, the operation should be deferred, almost until death is imminent, that the tumour be tapped and retapped, and when all chance of prolonging life beyond a few weeks remain, then the operation be performed. What is the percentage of mortality in these cases? We know that even many of these cases recover, notwithstanding the exhaustion of the patient, with the fluid in the cyst perhaps become purulent, not to speak of the multiplicity of adhesions set up by the frequent introduction of the

PROPOSED CLAMP.

SIDE ELEVATION



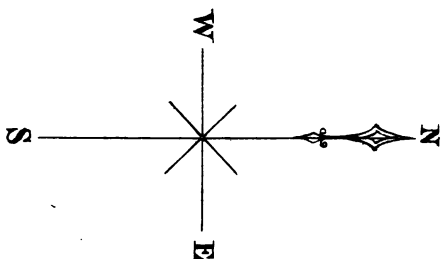
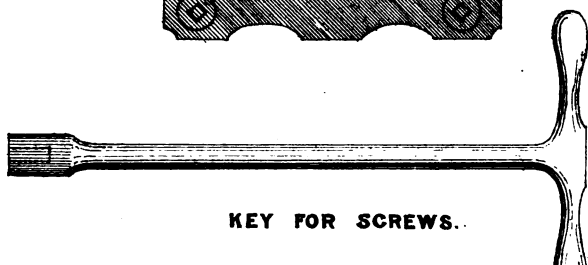
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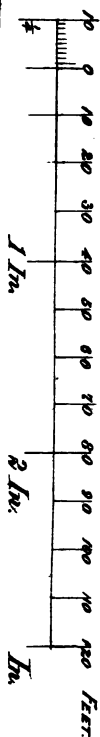
END ELEVATION A A.
LOOKING WEST



KEY FOR SCREWS.



SECTION THRU B B.
LOOKING WEST.



trocar in different parts of the abdominal wall. Would it not be a useful rule to lay down, that where, owing to the fears of the patient or her friends, it is determined to defer the operation as long as possible, and yet resort to it ultimately, the trocar be always introduced in close proximity to the previous insertion, and as nearly in the central line as the case permits of. Surely this plan would save much risk and difficulty afterwards in breaking down adhesions. Better still, when the patient has made up her mind to have the operation performed, to tap only once, and that more for the purpose of diagnosis, than to afford temporary relief.

With regard to the clamp, many different kinds exist, but they all seem to have one great drawback, they none of them grasp the pedicle uniformly; they either exert their pressure *entirely* from side to side, or from above downwards; the result being that the clamp does not come away until the seventeenth or eighteenth day. Some would argue that this is no objection, but rather beneficial, inasmuch as time is allowed for the more complete healing of the abdominal wound round the pedicle, as well as the more perfect shrinking in of the structure of the pedicle itself, before retraction takes place; but it is quite certain that as long as the clamp remains attached, great difficulty is found in keeping the parts beneath it clean, pus is apt to lodge thereabouts, and might find its way to the peritoneum. The drawing annexed represents a form of clamp which I am having constructed to do away with this difficulty, and when completed, only a trial will show how far it attains its end.

It consists primarily of 2 pieces, united by screws, when put together, the pedicle is grasped by two shaped bars, one of the bars being double, and between the two portions of it the other bar passes, as the key turns the screw. In this way the apices of the 2 pieces can be daily more and more approximated, uniformly compressing the pedicle at every point, until they meet and the pedicle is left free.

With regard to the treatment of the pedicle, even when very short, is it not possible to bring it outside the abdominal cavity in

very many cases where it is now returned. Where tapping has only been performed once or perhaps twice, the cyst wall is much more firm and unyielding as well as thicker in texture than when tapping has frequently been resorted to. I would suggest that in these cases, instead of ligaturing the pedicle and returning it into the abdominal cavity, in consequence of being very short, rather to include about an inch of cyst wall in the grasp of the clamp, place a strong ligature round the termination of the pedicle, also including the ends of the ligature in the clamp. In this manner, the risk of secondary hæmorrhage, which has so often occurred when the pedicle has been returned, would be prevented, as well as peritonitis; and retraction might be prevented by resorting to the plan of inserting an acupuncture needle through the structures in front of the clamp.

The last points to which I would draw attention is the antiseptic treatment. Should not the carbolic spray be always used during the operation, and whenever the wound is dressed afterwards—the cases of Mr. Barnes, Mr. Marion Sims, and my own would indicate its safety and ultimate success,—and afford a decided proof that no danger need be apprehended by allowing the carbolic spray to enter the abdominal cavity—whilst on the other hand, there is absolute certainty that no germs are permitted to come in contact with the peritoneum, the wound heals without suppuration, and perfect cleanliness to the parts is guaranteed.

It is in order to assist in elaborating some of these questions that I publish the only two cases of ovariectomy which I have performed.

CASE 1.—Mrs. S., residing at Didsbury, near Manchester, was admitted an in-patient at the Salford Hospital, on November 29, 1872. Visited her at her own home 9 months previously, found an ovarian tumour, enormously distended, which she said had been growing 7 years, and gradually increasing, but had never been examined by a medical man. Considered the tumour free from adhesions and probably unitocular. Tapped the cyst next day, and removed 7 quarts of thick dark coloured fluid; the tumour could then be easily discerned through the thin abdominal wall, and was almost completely emptied of fluid and easily

moveable. Had to tap again 6 months afterwards, and again 14 weeks after this. She was then much emaciated.

Operation performed on December 3rd, in the presence of my colleagues, Messrs. Stocks and Tomlinson, Dr. Crompton, and several other medical men. Temperature of the operation room 70, incision about 4 inches long. 21 pints of thick glairy fluid drawn off. Tumour very slightly adherent, chiefly to omentum, no hæmorrhage whatever. Pedicle long and thick, an ordinary shaped clamp used. Wound closed by sutures of silver wire passed through the entire abdominal wall, peritoneum inclusive, wound dressed with oil silk dipped in carbolic lotion, gauze, cotton wool and flannel roller: operation lasted 35 minutes. Tumour found to be unilocular, with large portion almost solid.

8 p.m., pulse 136, scarcely got over the chloroform; (2 hours after operation) complains of feeling faint, no vomiting, ordered ice, temperature of room (then 74°) to be kept down to 69°, a little beef tea every hour, a little burned brandy during night, and a grain of morphia in pill.

December 4th.—Pulse 124 regular, has had no vomiting, and a comfortable night, feels exhausted, to have a little iced champagne, and for food beef tea.

Dec. 4th, *evening*.—Pulse 116, temperature 102-2, has had no pain, or vomiting; to have another pill; morphia.

Dec. 5th.—Temperature 98, pulse 120, wound dressed in same way, no appreciable discharge, nearly healed, skin and tongue moist, slept about 3 hours, but felt quite easy all night. *Evening* Temperature 99, pulse 120, has had a little smarting this afternoon, otherwise quite comfortable, repeat morphia.

Dec. 6th.—Pulse 112, (no record of temperature.) Tongue furred, has some Tympanitis, probably owing to the liquid food, to have some thin rice pudding.

Dec. 7th.—Pulse 120, wound dressed, no appreciable discharge, end of pedicle black.

Dec. 9th.—All satisfactory, clamp quite firm, to have some fish.

Dec. 11th.—To have half-an-ounce of castor oil with 10 minims of Tr. Opii., pulse 125 rather fluttering, passed her water first time without catheter, wound quite clean, healed except round pedicle, clamp still firm, wound dressed with lint soaked in Tr. Benzoin. Co.

Dec. 16th.—No change since 11th until to day; the feeling of sickness, and the quick pulse now abated.

Dec. 17.—Clamp still firm—tied a ligature beneath it round pedicle,

Dec. 19.—Had 10 grains of Pil. Col. Co. C. hyoscyamo: yesterday, acted gently this morning, clamp still firm.

Dec. 22nd.—Removed clamp, which remained quite fixed.

Dec. 24th.—Get up yesterday first time.

Dec. 28th.—Discharged cured.

CASE 2.—Mrs. L—, aged 29, residing at Miles Platting, Manchester, married admitted into the Salford Hospital, September 19th, 1876, with an ovarian tumour. The cyst was tapped by me 18 months ago, cyst springing from right side. She then decided when the tumour re-filled to undergo the operation.

She menstruates regularly, but has grown much thinner since I first tapped her; and has never had any children. Could not get a uterine sound into uterus.

The operation was performed on September 28th, 1876, in the presence of most of my Colleagues, Mr. F. Southam, and several others. Incision 4 inches long from the umbilicus; 8 pints of thick dark, grumous fluid removed, of the consistency of pea soap. No adhesions whatever, pedicle very short, but managed to bring it outside abdominal wall, by putting on clamp at the very extremity; wound brought together by 3 catgut sutures. Carbolic spray (1 in 100) used during operation; and wound dressed with oil silk, gauze, carbolised plaster and many tailed bandage, operation lasted just 30 minutes she had a suppository containing half a grain each of morphia and Belladonna inserted in Rectum before being removed from the table. Tumour consisted of one large cyst, and a solid portion surrounded with small cysts, slight vomiting during the operation.

9 p.m.—Pulse 118, aspect calm, complains of an aching pain in abdomen, had $\frac{1}{4}$ grain of morphia hypodermically, to have no food, but iced water and barley water.

September 29.—Had been very faint during the night and some Beef tea given, vomiting occurring frequently until 7 a.m. then slept 20 minutes, vomited at 11 a.m. and again at 3 p.m. aspect dusky, severe cough commenced, not much pain, pulse 132, respirations 32, temperature 100·9.

Sept., 30.—Pulse 98, respirations 26, temperature 99·3; and in evening—Pulse 99, respirations 27, temperature 100·6. Is easy, but a good deal troubled with the cough, which occurs in paroxysms and causes great abdominal pain.

October 1st.—Complains of a great deal of pain in abdomen, attributed to the cough. No signs of peritonitis, no vomiting. Temperature 99·9, pulse 85, respiration 25; and in evening—temperature 99·9, pulse 92, respiration 25. Wound dressed under spray, all above the clamp healed; no discharge on the dressing.

Oct. 2nd.—Tongue clean and moist, still complains greatly of abdominal pain, pulse 83, temperature 100·1, respiration 24. Food chiefly consists of milk and barley water, and a little beef tea. Vomited at 10·15 last night.

Oct. 3rd.—Has had a good night in consequence of the cough subsiding, has had 2 grains of opium during the past 24 hours.

Oct. 4th.—Passed a good night. Continues pil. opii at night, wound dressed, no discharge, quite healed. Temperature 100·9, pulse 27, respiration 25. To have some rice pudding.

Oct. 5th.—Pulse 81, respiration 26, temperature 100·9. Sole and Bread for dinner.

Oct. 6th.—No action of bowels since operation, catheter still used—clamp quite firm—temperature 99·9, pulse 84, respiration 27.

Oct. 7th.—Much abdominal pain, a pil. opii given first time for 3 days—beef tea and rice pudding for dinner—temperature 101·1, pulse 89.

Oct. 8th.—She had a bad night, with a good deal of pain—probably flatus, pulse 80, respiration 27, temperature 100.

Oct. 9th.—Much easier. To have some Pulv Rhei c. Mag. and discontinue catheter.

Oct. 10th.—Temperature 99·2. Bowels acted at 3 a.m., and frequently during day.

Oct. 11th.—Temperature 98·6. Clamp still quite adherent, Ung. Zinci on lint beneath clamp, to heal the abrasion.

Oct. 17th.—Clamp still adherent, so was removed.

Oct. 18th.—To have ordinary food and get up.

Oct. 27th.—Discharged cured.

II.—A SUCCESSFUL MODE OF TREATING NŒVUS.

The surgical treatment usually adopted, and recommended in that form of nœvus which involves the entire tissue from skin to mucous membrane, is far from satisfactory. Caustic Irritants, various kinds of Seton and Ligature, removal by the knife, galvano and thermo canter, injection with carbolic acid, all have their advocates. The objections to the caustic applications, such as iodine and chloride of zinc, if they cure the nœvus, consist in the fact that ugly cicatrices in the skin ensue—setons, in large nœvi very rarely cure the disease; removal by cutting away the

diseased portion, is apt to be attended by serious hæmorrhage, and in a child by convulsions; the galvano cantery has not been attended by the brilliant results which were anticipated, and the attempts to set up inflammation in the part by injections of Carbolic acid and Perchloride of iron has more than once terminated fatally.

Two interesting cases of venous nævi, presenting in some degree the same characteristics have recently come under my care, in each case the disease extended from skin to mucous membrane—the one being an adult, with the nævus in the upper eyelid, the other an infant with the disease occurring in the upper lip. Both were treated in precisely the same manner, a mode of treatment which I do not remember to have seen recommended, and therefore perhaps worthy of record.

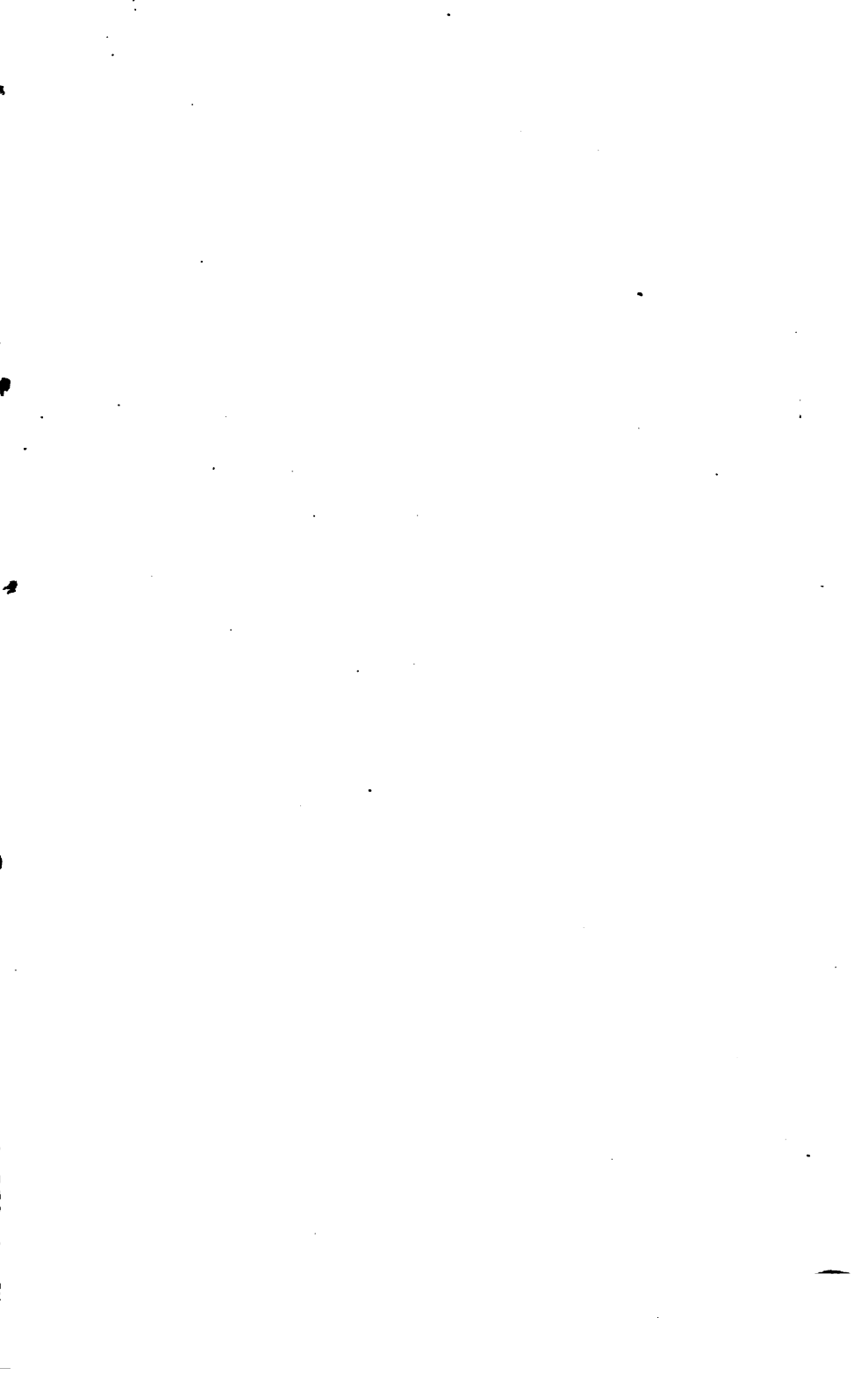
The first case was that of a child of wealthy parents residing in Rochdale.

This child was two years old, and previously to my seeing him, had been under the care of two medical men in Rochdale who I was informed, used local applications and setons. The case was then sent on to me by Dr. Crompton.

The entire upper lip was involved, and hung in an ugly swollen condition over the lower lip.

The nævus was not congenital, had recently developed, and as the parents stated, was increasing rapidly.

Assisted by Dr. Crompton, I passed 3 acupressure pins horizontally, at equal distances, and parallel to each other. After puncturing the skin with the point of the pin I passed it on in a rotatory kind of way, something like darning a stocking, backwards and forwards, taking care to keep the point away from the skin and mucous membrane, until it emerged at the opposite side of the lip. Endeavouring in this way to secure compression of all the veins, and at the same time to include them all in the grasp of the pin. This I believe would have been sufficient, but in order to make quite certain I passed a ligature of worsted, in the figure of eight fashion, over the pin, but not sufficiently tightly to impede the circulation in the skin. The worsted was allowed to remain 3 or 4 days; but the pins were not removed for a fortnight, the parts gradually shrunk, the veins became obliterated, the darkened purplish skin resumed its healthy hue without any cicatricial marks—and in about 8 weeks the lip had so far resumed its natural size and shape that it would have been difficult to trace any sign of the nævus. No convulsions, local inflammation or symptoms of constitutional irritation at any time supervened.





The other case occurred in a young woman whose appearance "before and after" is shown in the accompanying illustration, and was congenital, but she always trembled at the thought of an operation. She was admitted into the Salford Hospital and I treated her case in exactly the same way as the previous one. The only difference in the progress of the case being that slight erysipelatous inflammation over the forehead and temple, showed itself on the fourth day, but this quickly subsided on the removal of the worsted.

This method of procedure seems to me so highly satisfactory in its results, so entirely free from any kind of danger and at the same time so eminently simple, that it will probably meet with general approval.

NOTES ON THE PATHOLOGY OF PLASTIC BRONCHITIS.

By **RICHARD CATON, M.D.,**

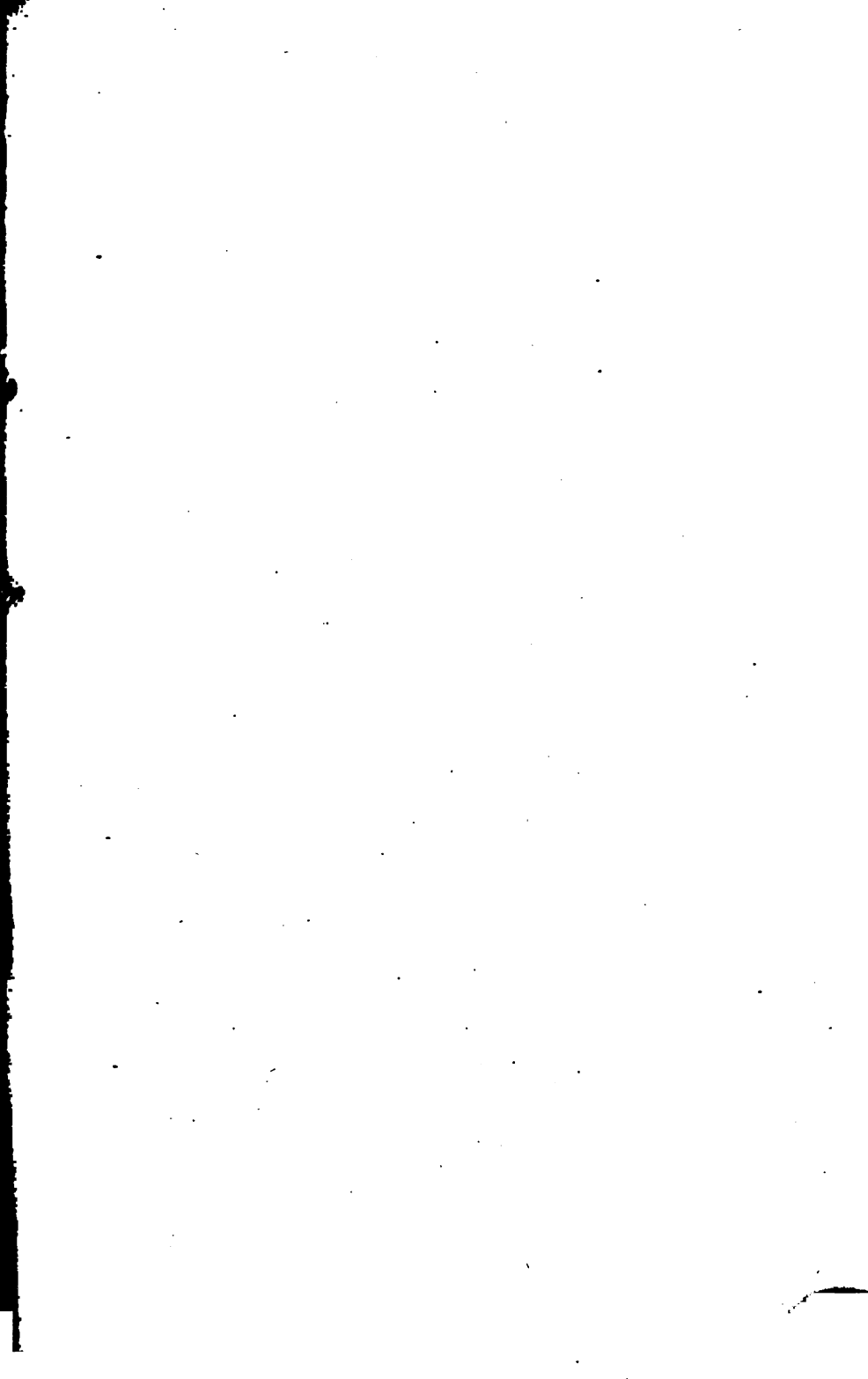
Lecturer on Physiology, Liverpool Royal Infirmary School of Medicine.

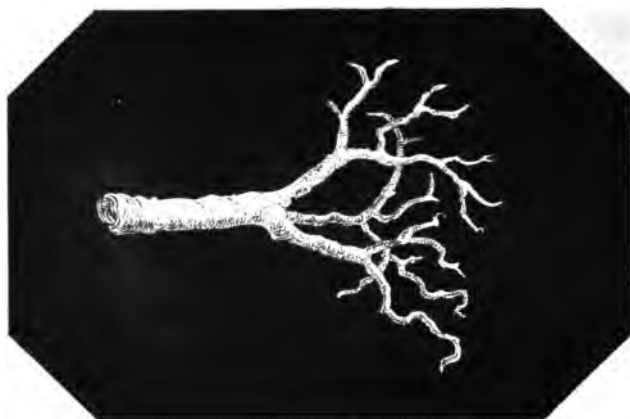
Physician to the Liverpool Northern Hospital.

THERE is a form of Bronchitis in which, in addition to the changes in structure and secretion characteristic of ordinary cases of the disease, a delicate membrane is formed upon the epithelial surface of the bronchial tubes.

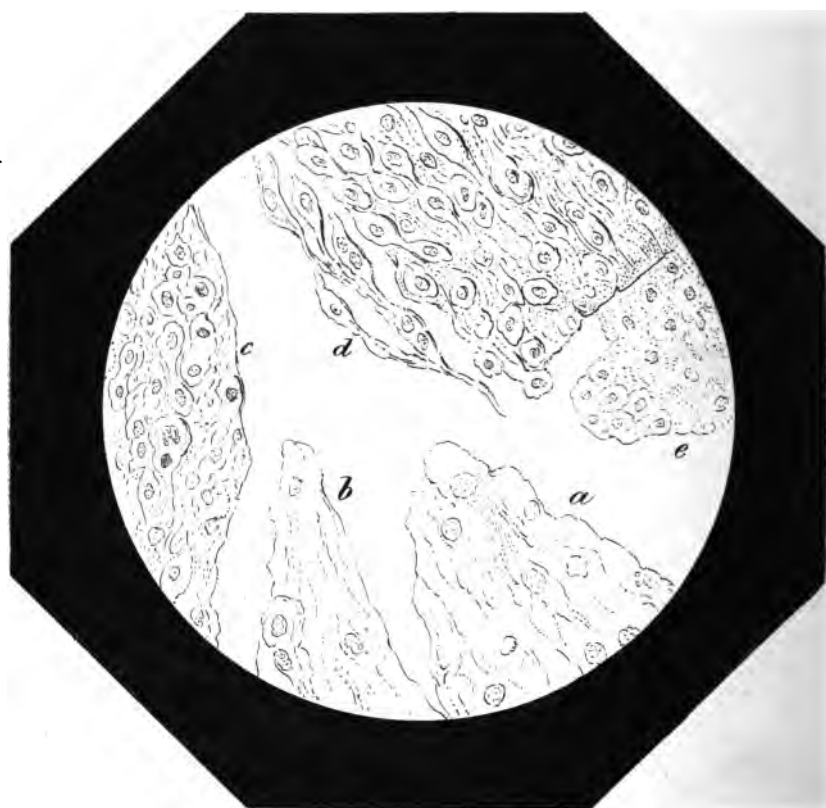
The disease has been described by various writers under the names of plastic, pseudo-membranous, fibrinous, or croupous, bronchitis; but owing to its comparative rarity, no very careful investigation has yet been undertaken of its pathology.

The last important paper which has appeared on the subject in English journals, is one published by Dr. Peacock, in the *Pathological Transactions* in 1854. He had collected notes of thirty-four well authenticated cases, in addition to nineteen incidentally noticed by various writers. Since the date of Dr. Peacock's paper several cases have been reported in the *Pathological Transactions* and in various English journals, and a large number have been collected from Foreign journals, and Treatises by Prof. Biermer, and are noticed by him in *Virchow's Handbuch*.





CAST FROM A CASE OF PLASTIC BRONCHITIS.



MICROSCOPICAL APPEARANCES.

During the winter of 1874-75, a case came under my observation at the Liverpool Infirmary for Children.

The patient, a boy five years of age, was admitted with symptoms of somewhat severe bronchitis, attended with the expectoration, at intervals of one or two days, of tubular branched membranous bodies, an inch and a half in length, from the air passages.

On examination of the chest, no dulness was discoverable, but loud and coarse moist râles were to be heard in nearly all parts. The attacks of cough usually resembled those of ordinary bronchitis, but occasionally they were of great violence and accompanied with much dyspnoea and distress, the lips and face becoming blue; at those times he brought up one of the casts. Ordinarily there was no special dyspnoea.

Under the usual treatment he recovered, the cough and expectoration entirely ceasing.

During the succeeding winter, he took cold, and again appeared at the Infirmary, suffering from severe cough and the expectoration of similar casts. On this occasion, in addition to moist râles, some patches of slight dulness were discoverable.

The casts were brought up in considerable numbers, sometimes more frequently than once per day.

The boy's general health did not appear to be seriously impaired, his appetite continued good, and he was never confined to bed.

I was unfortunately unable to learn the ultimate issue of this second attack, as the patient left the Infirmary and I have not succeeded in tracing him.

The upper of the two accompanying lithographs represents the general appearance of one of the casts when floated in water. It is not an exact copy of any individual cast, for they were generally more or less torn and injured, it is rather a restoration, drawn from the uninjured parts of two or three, and is rather larger than the actual size.

The casts are soft in consistence, and when examined in transverse section are seen to be composed of several concentric layers of membrane.

I stained numerous portions of the membrane with carmine, logwood, or silver, and examined them microscopically. The appearances varied somewhat in different specimens. The most general character is that represented at *a* and *b* in the accompanying

lithograph. The fineness and delicacy of the membrane is so great that I found it impossible to represent it adequately in my drawing, and it is still less preserved in the lithograph. There is rarely any trace of fibre, but a considerable number of round or oval nuclear bodies which tint strongly with carmine; occasionally, as at the lower part of *b*, these are seen to be surrounded by encircling lines which show them to be the nuclei of cells.

At *e* is represented a specially delicate bit of membrane part of which appears to be exclusively composed of cells joined by their edges. In other parts the cells appear to have coalesced so completely that the markings of their edges are almost or entirely lost.

At *d* the membrane is thicker and presents distinct cell outlines many being spindle-shaped, at one edge some of these cells, by elongation in the direction of their poles, have formed fibres.

At *c* the cells are abundant and convey the impression that they are undergoing division, several have two or even three nuclei, and in some instances the general cell mass appears to be in process of cleavage. Cell growth seems to be taking place here by proliferation.

I give these details because the exact nature and mode of origin of these bronchial casts is at present enveloped in much obscurity. No pathologist has, so far as I have been able to discover, published any minute description of the microscopical appearances. Prof. Biermer, who devotes twenty pages of small type to this disease in Virchow's Handbuch, merely describes it as "a structureless, somewhat fibrous looking basis, incrustated with cellular inflammation products of the mucous membrane."

It is difficult to determine what is the real nature of the membrane—whether it is simply a fibrinous exudation in which leucocytes have been accidentally imbedded, or whether the membrane is entirely composed of modified cells. The former seems to have been the opinion of most writers though expressed with more or less vagueness, and although contrary to the general principle that fibrinous exudation does not occur on mucous membranes.

The occasional discovery of a portion of membrane like that shown at *e* inclines me to the hypothesis that the membrane may have originated solely from cells. Not however exactly in the same manner in which Wagner has shown the croup membrane to arise, for the epithelium in the bronchial tube is columnar, and I rarely, if ever, find columnar cells in these casts. In sections of the inflamed bronchus multitudes of leucocytes are to be seen crowding the dilated vessels and scattered thickly through the whole substance of the mucous and submucous layers: moreover, on scraping the epithelial surface of such a bronchus, many cells are found lying free.

If we can imagine their being poured out in great numbers, or generated in the epithelial layer, and further, suppose them to be in a plastic condition, so that they adhere together, forming a membrane, and that they subsequently lose many of their nuclei, a change not uncommon in normal and pathological cell-membranes, we shall have a fairly satisfactory explanation of the mode of origin of the casts in question.

It appears that each membranous layer when formed is detached from the mucous membrane and a second forms round it, which again is enclosed by a third and perhaps by a fourth, until the mass interferes so much with respiration, that strong expiratory efforts are made, which finally detach and expel the whole cast.

From analysis of the collected cases it appears that the disease is more common in the male sex, and that it attacks children more frequently than adults; it is usually acute, but occasionally assumes a chronic form, lasting for five, ten, or even a greater number of years. In a few instances death has occurred suddenly from impaction of a cast in the larynx while being coughed up. Usually the disease does not appear to be specially dangerous.

The treatment does not differ from that of ordinary bronchitis.

NOTE ON AN IMPROVEMENT IN THE SHORT HINGED FORCEPS.

By FRANCIS VACHEB, Birkenhead.

IN a brief paper contributed to the '*Liverpool and Manchester Medical and Surgical Reports*, 1874,' I detailed certain improvements in the Hinged Short Forceps which some experience with the instrument had enabled me to make. I removed the clams a little further from the handle by the introduction of short shanks, added a head to the hinged rivet or pin, to allow of its being readily drawn and arranged that the steel snap which locks the blades should be controlled from the end of the handle instead of by a button liable to be pressed on by the hand of the operator.

Thus altered the forceps at first appeared to me to fulfil all the conditions required, but increased experience revealed to me yet another defect, to remedy which I had again to consult with Messrs. Weiss & Son. The blades when brought into position are maintained in their places and the two halves of the handle fastened together wholly by a steel snap. Now as the palm of the hand of the operator, when making traction, is in contact with the side of the handle opposite the hinge, it will be obvious the strain on the spring snap is considerable. I am not quite sure whether this imperfection was primarily presented to my mind as a theoretical one, but I know that on a certain occasion when more than

usual force was applied the snap yielded, and the necessity of modifying the instrument in this respect was demonstrated. The desideratum, a lock giving absolute security, might have been obtained in many ways; for instance by increasing the size and strength of the snap, or by an appliance for bolting the instrument in the open position; but the plan I have adopted appears to me to be the simplest and best. The end of the hinge pin has been made to screw in, and to the transverse part of the steel snap a little collar has been added, which encircles the neck of the pin. After the instrument is locked the pin can therefore be screwed down on this collar. Three turns of the screw fix the snap, and three in the reverse direction release it. The hinge pin is not less easy to remove where that may be necessary, and when screwed down the handle and blades are as firmly fastened in the open position as if they were not made to fold.

This little alteration in the Hinged Short Forceps was effected for me in November, 1874. I did not publish an account of it at the time, preferring to wait till opportunities for testing it had been afforded, and thinking it just possible that if I waited I might have some further modifications to suggest. As now two years have elapsed, and I see no reason for being dissatisfied with the instrument in its present amended form, and believe that alteration herein described is essential, I judge it expedient to submit this note for the information of the profession.

REMARKS ON VENESECTION WITH CASES ILLUSTRATING ITS VALUE.

By T. B. GLYNN, M.B. London, M.B.O.P.,

Physician to the Royal Infirmary

IN this age we cannot fail to recognize the prejudicial effect bleeding must have exercised in a large proportion of the diseases for which it was formerly practised; we realize the disastrous results which the belief, that the lancet was indispensable in all cases, where there was a quick pulse and hot skin, must have entailed; and shudder as we picture an unfortunate with pyæmia, rheumatism, or continued fever, bled again and again with each returning rigor, each exacerbation of his disease; we understand the nature and significance of the asthenic condition produced by the loss of a large amount of blood and marvel at an infatuation which could lead to the removal of such quantities as four or five pints in three or four days.

The reaction, however, against the extravagances of our predecessors, has carried us further perhaps than physiological teaching or clinical experience warrant; and has driven us, the younger generation of practitioners, too often to neglect a remedy of immense value in certain cases. Modern research has not only given

us new weapons, with which to meet disease, but has taught us how to use, and showed us new virtues in many old ones. Although venesection can no longer be regarded as "the remedy and the only remedy for inflammation" (Marshall Hall,) or although considered an antiphlogistic can rarely be needed as such, when we have aconite, quinine, salicylic acid and the bath; it is very useful, I believe in certain affections of the lungs and heart attended with engorgement of the right cavities of the heart and venous system. I shall attempt to show this by referring to certain cases of bronchitis and heart disease, lately under my care where I have practised bleeding. I wish also to record a case of renal disease with uræmia, where blood-letting was of great service.

Those of my readers whose experience extends over 30 or 40 years, who recollect the Sangrado like doctrines in vogue during their earlier days when blood was drawn as if "*le secret de guérir toutes les maladies était renfermé dans la saignée.*" Those who have practised venesection in cases similar to those to which I shall presently refer to, and have witnessed the relief it has afforded; may be amused at my advocating bleeding as if it were an untried remedy. They must, however, remember that although blood-letting is occasionally recommended in modern writings, it is very rarely practised; that by many it is regarded as a deservedly obsolete remedy, and, that to many who like myself are conversant only with the current practice, it is really a novel procedure.

One of the most striking results of venesection is the relief it affords to dyspnoea in certain forms of cardiac and pulmonary disease attended with venous engorgement. This probably depends upon its reducing the mass of the blood and lessening the resistance in the right side of the heart. Traube, however, believes that it is due to its diminishing their irritability of the respiratory centre. I do not intend to discuss the general effects of blood-letting, but must refer to this question, for if Traube's explanation is the correct one, then must any alleviation be dearly bought, for to reduce the irritability of the respiratory centre is to lessen its capacity for

recognizing the respiratory wants of the system and to induce death by apnoea. The action of blood-letting, in diseases where there is venous engorgement, and in health must be very different. Under the former circumstances, the removal of venous blood by diminishing the resistance and lessening oedema quickens the circulation through the vessels and so stimulates organs. Under the latter the abstraction of blood lessens the blood pressure and so depresses them. Indeed, from clinical experience I am satisfied that venesection in the cases I advocate it exercises a stimulating effect generally, the irritability of the medulla oblongata, at any rate of that part of it which is the centre for the reflex act of vomiting, is increased. I have, in suffocative bronchitis, seen emesis excited by one drachm of ipec. wine, after bleeding, where an ounce has failed to induce it before. In suppression of urine I have witnessed blood letting immediately followed by action of kidneys, as in this case.

D. Mc C., aged 39, suffering from chronic renal disease following acute nephritis 12 months before,—had been in the Infirmary several days, the amount of urine all the time diminishing; on February 7th, 24 ounces were passed in 24 hours; on the 9th, 20 ounces; 12th, 16 ounces; 13th, 11 ounces; 15th, only about two teaspoonsfuls. During this time he took various diuretics such as compound jalap powder; inhaled juniper oil, was wet cupped over the loins and had hot air baths without any benefit. On the 15th, 15 ounces of blood were taken away from the arm; the day after, he passed 26 ounces in 24 hours; on the 18th, 27 ounces; 19th, 32 ounces.

It is probable that the effect of blood-letting in reducing the mass of the blood is only temporary, that the volume of the blood is soon restored by absorption from the tissues; however a temporary fillip may be given to organs, immediate danger may be averted, life prolonged and time gained for the administration of remedies.

The great evil of venesection is that it render the blood poorer and thinner, and in the diseases for which bleeding, as a mechanical agent is of service, the blood is already impoverished and further exhaustion favours serous transudation. Misgiving on account,

should not I think prevent our using the lancet in certain of those urgent cases, where patients are dying from an aggravation of their symptoms owing to distension of the heart, engorgement and œdema of the lungs, etc.

In the examples of bronchitis and mitral disease which I have selected in illustration, there was considerable anasarca, the depletion far from augmenting the dropsical symptoms was followed by their rapid decline, and with increased excretion of urine. The tendency to dropsy favoured by the poverty of the blood, was more than compensated for by the invigoration of the circulation and improved renal action. The administration of digitalis given in these cases, after venesection as a vaso-motor tonic has, I believe, contributed to the favourable result. I may here, refer to a case of spontaneous hæmorrhage as illustrating the effect of loss of blood on respiration. For the notes of my cases I am indebted for the most part to my clinical clerks, Messrs. Williams and Clegg.

J. M., a male was admitted into the Royal Infirmary, suffering from aneurism of the thoracic aorta, suddenly became collapsed, and was for some hours at the point of death; no blood was coughed up, but from his appearance it was evident that the aneurism had ruptured, he rallied and next day we found the left side absolutely dull from summit to base with the exception of a little tracheal resonance under the clavicle, there was also absence of vocal fremitus and of respiratory murmur. The diagnosis was that the aneurism had given way and produced hæmothorax. The man lived two or three weeks after the accident, gained colour and strength very rapidly and finally died of a recurrence of hæmorrhage.

From first to last although one lung was more or less completely collapsed, as was found after death, the respirations never exceeded 13 or 14 in the minute. When a lung or a part of one becomes suddenly incapacitated, as in this instance, by an acute lesion, it is evident that the removal of blood is calculated to relieve dyspnœa.

Case of aneurism of the aorta, or of the innominate artery pressing on the trachea.

T. D., age 38, engaged in a brewery, admitted into the Royal Infirmary, March 22nd, 1877, suffering from intense dyspnoea and cough. "He is a tall well-made man, no history of rheumatism or syphilis; was carried upstairs, expression anxious, voice weak and broken, complexion livid, at one time sits up in bed stooping forwards with his head bowed down as far as possible, at another throws himself back, as if exhausted, breathing hurried, inspiration and expiration laboured, noisy and frequent, cough constant, muffled, much frothy expectoration. Physical examination, slight pulsatory movement of upper part of sternum and sternal end of clavicle. Much pulsation and resistance to finger in supra sternal notch. No murmurs, but the cardiac sounds are loud, over manubrium—second sound intensified, nothing of moment discovered on the examination of the lungs or heart. By the laryngoscope there did not appear to be any paralysis or spasm of the vocal cords." The diagnosis was aneurism pressing on the trachea. "Ordered ice bag over sternum Ext. Opii half grain Potassii Iodidi 15 grs. in a mixture."

"March 23rd.—No sleep—very restless. The dyspnoea became aggravated after taking a pill, so had only one. Has taken nothing but a little tea. Respiration 35, pulse 130. Appearance and posture the same as on the preceding day, but he was more exhausted. Cough constant—had expectorated two large cupfuls of frothy mucus." I ordered him to be bled. The blood was very dark and ran so slowly that the veins were opened in both arms, thirty ounces were taken away. He expressed himself as much easier immediately after the operation. An hour or two later, the note is, "lying low half propped up by a bed rest has been semi-recumbent since he was bled and has slept an hour and half, taken an egg, tea, and toast. Respirations much easier, 20; pulse 120."

March 24th.—"Has been able to lie on his back all night—has slept well from time to time. Lies low with three pillows, very little lividity. Expression easy, cough much less frequent and strains him much less. Can swallow without discomfort, takes food much better. Ordered a restricted diet and to continue the iodide."

25th.—"Slept nearly all yesterday, and well last night."

26th.—"Is very comfortable, cough less, expectoration much diminished."

April 3rd.—"Continues to improve. Pulse, 72; respiration, 15."

April 9th.—"Passes two or three hours without coughing; expectoration nearly ceased. The physical signs of aneurism much as before, but there is now a systolic murmur over the area of pulsation."

This man is still under my care, he has very little discomfort but the aneurism has not yet consolidated.

Venesection was here of the greatest service in relieving the urgent symptoms. The relief afforded may in a great degree have been due directly to reduced pressure on the trachea, consequent on the mass of blood being diminished and the aneurism lessened.

During the colder months, more particularly in hospital practice we meet with very aggravated cases of bronchitis and emphysema; patients are from time to time admitted gasping, blue, cold, dropsical and almost pulseless, their winter cough and their breathing have, they say, been much worse of late. Several nights they have spent in a chair, their legs have swelled rapidly, their urine is very scanty, they are now occasionally delirious and their friends at length alarmed, bring them to the hospital; bring them when it is frequently too late to give relief. I have attained better results in these cases, by bleeding and giving an emetic than by the administration of alcohol, ammonia, and by counter-irritation alone. Sometimes, patients are so exhausted or in such an asthenic condition with thin blood, renal disease, and excessive anasarca, that such rigorous treatment would be prejudicial or would not offer a chance of success. Patients in the condition I have described do not bear alcoholic stimulation well, and in this they differ from those with pneumonia. The odour of the spirit is readily detected in the breath, indeed, it would be quite possible to diagnose cases of bad bronchitis and emphysema by the nose alone, from the fumes of brandy and æther, which generally surround them. There is frequently much thirst, in these cases, this and the sense of weariness provokes a craving for fluids and stimulants, so that large quantities of brandy, etc., may readily be administered; I believe, however, that the dyspnoea is exaggerated by any but a small dose of alcohol and the cerebral symptoms (delirium or stupor) increased.

Any who have given emetics in cases of this kind know how difficult it is to produce vomiting. I have given on different occasion large doses of ipecacuanha wine, sulphate of zinc, tartar emetic, sesquicarbonate of ammonia, and once injected apomorphia without producing any decided effect; but have seen the abstrac-

tion of from 10 to 15 ounces of blood, not only relieve the dyspnoea, but accelerate the action of the emetic in a marvellous way. This is, I think, to be ascribed as I have mentioned before to the restoration of the irritability of the nervous centres concerned, by the quickened circulation of arterial blood through them, consequent on the lessened resistance of the venous. I will now illustrate these remarks by referring to one or two cases.

"W. F., age 51, seaman, admitted into the Royal Infirmary, February 14th, 1877. Was carried into the ward and propt up in bed. Complexion dusky, lips blue, hands and feet cold, legs œdematous, pulse almost imperceptible, urine very scanty, back round, shoulders high, soft parts about chest sinking in on inspiration. Heart's apex beating in epigastrium, liver lowered, expiration much prolonged, respiratory murmur almost inaudible over bases where there were mucous râles. Was cupped to 5 ounces. An emetic of ipec. wine, was ordered with a mixture containing tincture of digitalis ten minims, and potassio tartrate of antimony the eighth of a grain, a dose. Milk diet and gin 3 ounces.

Feb. 15th.—"No rest all night, felt his mind wandering, is propt up in bed. Complexion more dusky, limbs tremble, breathing laborious, all extraordinary muscles of respiration called in action, veins in neck swollen, pulse almost imperceptible, respirations 32, cough very troublesome, expectoration scanty nummular, but more than on previous day. Tongue blue, dry; urine, a trace of albumen, 14 ounces. Bowels acted once. The ipec. wine yesterday produced no vomiting, (an ounce was given in drachm doses; at 2.15 p.m.) Ten ounces of blood were taken away from him, the emetic was repeated after venesection, he vomited freely on taking one drachm of ipec wine, and ejected a considerable quantity of viscid mucus. 5 p.m.—Face livid, breathing easier, pulse more perceptible, 104; respirations, 38.

Feb. 17th.—"Had a much better night, slept at one time for 3 hours, complexion less livid, says he can take a much "deeper breath," pulse much fuller 100, breathing easier, respiration 24, cough easy, expectorated a large quantity of thick purulent matter. Takes food better, tongue moister, urine 20 ounces, a trace of albumen. Physical signs—respiratory sounds, behind, over bases very feeble with small mucous râles, in front, exaggerated, over upper parts of lungs, feeble over lower where there are small mucous râles.

Feb. 18th.—"Had a better night than he has had for a long time, slept 9 hours nearly, only being roused for food, and by cough, has expectorated a large quantity of muco-purulent matter, breathing easy, pulse fuller, 100, bowels acted

once, urine 26 ounces. On this day, was ordered brandy six ounces, instead of the gin, and one half dose of the mixture every three hours." He steadily improved, and on the 22nd, was ordered a chop, was able to lie down in bed and passed two pints of urine. On the 26th, he left the hospital."

"D. H., aged 54, labourer, admitted January 7th, 1876, suffering from bronchitis and emphysema. With the exception of winter cough has enjoyed fair health till 2 months ago, when present illness commenced with cough, copious expectoration and pain at sternum. He is now unable to lie down, his complexion is bluish, legs œdematous, breathing hurried, cough constant, expectorates a very small quantity of transparent adhesive mucus, pulse very small, 90, temperature low, urine very scanty, contains a little albumen. No alteration in resonance on percussing chest, dry râles anteriorly and posteriorly. Ordered, milk diet, beef tea, and 4 ounces of brandy, to be bled to 8 ounces, an emetic of ipec. wine and counter irritation to chest.

"Jan. 8th.—Has had a very bad night, tossing about in bed, continues to feel sick today, has slept a little this morning, is still unable to lie down, complains of the pain and 'tightness at chest,' breathing very hurried and laborious, urine scanty, passes pure blood per rectum. The emetic to be repeated, to be bled 4 ounces and a mixture containing carbonate of ammonia, tincture of digitalis and æther every three hours. 7 p.m.—Pain in chest decidedly relieved, is quieter, less lividity, breathing easier, expectorated much thick mucus.

Jan. 9th.—"Slept occasionally during the night, lividity of countenance less, looks easier, pulse fuller, 80; respiration, 30 easier, cough very troublesome, expectorated much mucus. Urine increased decidedly, but not measured as bowels acted three times.

10th.—"Had the best night he has had since he has been in the hospital—looks more comfortable, pulse improved 75, respiration much easier, expectorated 2 cupfuls of mucus, urine 15 ounces, bowels acted once; physical signs as before, respiratory sounds more audible behind.

11th.—"Much the same as yesterday.

12th.—"A good night, cough less, is able to lie down occasionally, urine 25 ounces, albumen less.

14th.—"Much improved, cheerful, says that the cough does not shake him so, respiration, 25; pulse stronger, 75; urine 30 ounces; physical signs, respiratory sounds more marked behind, expiration less prolonged.

15th.—"Going on well, ordered fish."

17th.—“A bad night from cough, wishes to get up, urine 30 ounces, bowels acted.”

19th.—“Out of bed, cough better, pulse 80, urine 40 ounces, feet rather swollen.” He gained strength rapidly, and was convalescent on the 24th.

I might refer to other cases, where similar treatment has been equally successful, or where, though the patient has eventually succumbed, life has been decidedly prolonged.

The blood removed, has been very dark in colour—has flowed very slowly, and in one case was in the condition described by Dr. George Johnston, it was like treacle in appearance and consistence, no blood ran from the vein in the arm when incised, the external jugular was then opened, but only a very small quantity, escaped, to get more it was necessary to squeeze it out, the case was one of bronchitis, such as I have described, and the man was nearly moribund. However he was the better for the bleeding, but being next day taken out of the hospital was lost sight of. It is extraordinary that the circulation and life was maintained at all with the blood in such a condition.

I have found blood-letting associated with the administration of digitalis of great use in certain cases of mitral regurgitation, more particularly in those where the blood is not yet markedly impoverished, but where the heart perhaps from an attack of bronchitis has become suddenly embarrassed, after a bleeding in such cases the effect of the tincture of digitalis in fifteen minim doses in strengthening and steadying the pulse has been most marked. Sometimes when I have not felt justified in taking blood from the arm, I have seen great benefit follow the application of half a dozen leeches to the chest.

The following case was of such a serious nature, the asthenia was so marked; there being much dropsy, that I for some time feared to bleed.

“M. E., aged 33, admitted into the Infirmary, January 16, 1877. Has had many attacks of rheumatic fever, the last 3 years since. About 9 months ago commenced to suffer from palpitation, dyspnoea, cough, and frequent vomiting.

A fortnight ago became much worse, and seven days since the feet began to swell. Has been addicted to spirit drinking. The face is cyanotic and oedematous conjunctive yellow, veins in neck swollen, tortuous and pulsating, legs anasaruous, skin tense, abdomen distended, tympanitic over central regions, dull over the lateral, radial pulse imperceptible, breathing very quick and laboured, cough troublesome, general heaving impulse over heart, difficult to localize apex beat. Loud systolic murmur at apex, also a systolic murmur at base audible up carotids; ordered ten minims of tincture of digitalis every three hours."

17th.—"A bad night, propt up in bed. General symptoms much the same as yesterday, mucous râles and feeble respiratory sounds over both bases behind. Carotid pulse 160 irregular, radial pulse cannot be counted. Respirations 38, cough very troublesome, urine 4 ounces, sp. gravity 1020, trace of albumen, bled from the arm to 10 ounces." Immediately after venesection the note is, "breathing perceptibly relieved 32, pulse 168."

18th.—"A restless night, but patient lying low in bed, legs have lost shiney appearance, hearts action stronger, murmurs louder, breathing easier 28, respiratory sounds clearer at bases, radial pulse stronger, urine 6 ounces, the bowels have moved twice."

19th.—"A much better night, face no longer cyanotic, lips pink, veins less distended, pulsation less, abdomen smaller, respirations, 20, urine, 11 ounces.

20th.—"A good night, veins in neck scarcely visible, pulse 100, respiration 24; urine, 10 ounces.

24th.—"Going on well, pulse 96, respiration 24, urine 37 ounces. From this time continued to improve, and on Feb. 25th, the note is--That anasarca is nearly gone, urine 96 ounces." Soon after patient left the hospital in fair health.

"T. B., age 40, admitted April 24, 1877. Mitral disease and anasarca. He had rheumatic fever 20 years ago—a cough for a year or two, breath short for several months. He has been propt up in bed four or five weeks, his legs have swelled very much, and he has passed very little urine. He is supported in bed by a bed rest, his face is flushed and cyanotic, expression anxious, conjunctive yellow, lips and tongue bluish, veins in neck distended and pulsating, arteries in neck beating visibly. There is general anasarca, respirations 28, expiration prolonged, cough very troublesome, expectorating frothy mucus, pulse at wrist almost imperceptible, irregular 80, impulse of heart diffused, apex beat in 6th intercostal space just outside nipple line, area of dulness extends across sternum, sounds clear at base, systolic murmur at apex, urine very scanty sp. g. 1022, albumen one third, ordered fifteen minims of tincture of digitalis every three hours; and seven ounces of blood were taken away.

26th.—“ Had a much better night, lividity less, pulse stronger, says he feels more comfortable, ‘ but weak from loss of blood,’ anasarca less, urine 20 ounces.”

27th.—“ Looks very much better, still propt up in bed, complexion less livid, expression easy, pulse much fuller, regular 80, respiration, 20; area of cardiac dulness diminished.”

28th.—“ Much better this morning, but propt up in bed, cough still troublesome, pulse stronger, 78; breathing quiet.”

30th.—“ Pulse, 60; respiration quiet, cough much better, is able to lie down in bed, anasarca nearly gone, urine 102 ounces, not albuminous.”

May 4th.—“ Out of bed, anasarca gone, cough much better, pulse, 75, somewhat irregular but of fair volume, urine 70 ounces, not albuminous. Systolic murmur at heart’s apex as before.” A few days afterwards he was discharged.

I have occasionally practiced venesection in renal disease with urœmia. It sometimes has exerted a marked influence over the symptoms. I shall refer particularly to one case. With our imperfect knowledge of the cause of the so-called urœmic attacks, it is impossible to determine how the abstraction of blood operated, whether by lessening the amount of poison in circulation, or by relieving cerebral anæmia, either by diminishing in any way arterial spasm, or by promoting absorption and reducing œdema of the brain.

The following case is very fully reported by my late clinical clerk, Mr. Clegg, but as it is very lengthy I shall curtail it as far as is compatible with the preservation of a faithful record of it.

C. D., aged 42, an engineer, admitted October 14, 1876, suffering from renal disease, probably contracted kidneys. The most important facts in the history of the case are “ That he had an attack of gout 12 months before and shortly after this an apoplectic seizure, when he remained insensible three days, about this time his sight became bad. He sees things as it were through a cloud and also has from time to time had attacks of transient blindness. He complains of constant headache, and nausea, says that he passes too much water, gets up about four times a night to do so. Expression worn and anxious, face pale, radial arteries very hard and tortuous pulse not obliterated by pressure of 800 grammes of sphygmometer, 84; sphygmographic tracing showed great tension; resp., 16; urine pale, 60-70 ounces in 24 hours, sp. gravity 1011, albumen, 1.5; contains a very few small granular casts; heart’s apex beats in 6th space just inside nipple line, area of cardiac dulness increased, systolic murmur at apex, accentuation of 2nd sound at base. Ophthalmoscopic examination showed advanced albuminuric

irititis. Ordered an iron mixture, occasional purgatives, and bromide of potassium 20 grains at night. He continued much the same for the next few days, suffering chiefly from headache, the iron was omitted, and tinct. of digitalis and bromide of potassium given with advantage. He inhaled a drop or two of nitrate of amyl about this time, it exaggerated the headache and increased the dimness of sight. During the early part of November he became decidedly worse, he was troubled with boils, had a slight bed sore, the headache was constant, with much nausea, his memory was more impaired, he grew very deaf, and could only read the largest letters on the board at the head of the bed, he became very irritable, sometimes talked incoherently. Temperature normal, or rather below, urine on November 10th, 90 ounces in 24 hours, contains 293·17 grains of urea.

November 14th.—“Reported that he felt the left arm very weak, that he constantly sees phantoms of things, he has been accustomed to see, that he lay awake all night watching these, the bed clothes feel to him as if made of satin, urine 50 ounces in 24 hours, contains 239 grains of urea. Diet: milk.

24th.—“His temperature fell to 97·4 fahr. The next day was incoherent in the morning, and had to be held in the bed, vomited after breakfast, and was seized with a rigor, temperature at 9 a.m., 97 fahr. At 10.30, face pale, no oedema lying curled up on left side with his eyes shut, muttering to himself, will not answer questions or protrude tongue, muscles of extremities constantly twitching, swallows with difficulty and only when fluid reaches the back of the throat, the right foot and arm when pricked are withdrawn, not the left, does not follow a light held before eyes, dense white fumes are given off from a rod dipped in hydrochloric acid held before mouth, pulse very hard, 96, strong cardiac impulse respiration 20, urine 40 ounces. Ordered croton oil, which purged him, and six leeches behind the ears. As the day advanced he became more unconscious. He moaned constantly and was terribly restless, sitting up and lying down, occasionally vomiting, but all the while insensible to external things. No urine passed. His temperature gradually rose and pulse quickened, at 12 temperature 97 fahr., pulse 108, at 4 p.m., temperature 99·4 fahr., pulse 112, still hard. At 9 p.m., ordered to be bled to 15 ounces. After bleeding, patient quieter pulse 136, softer, temperature 99·8 fahr.

25th.—“Very much better, seems quite comfortable, is quite rational, says he is sore all over. Temperature at 9 a.m., 101 fahr.; at 9 p.m., 100 fahr.; pulse, 116; respiration, 20. Ordered tincture of digitalis in 20 minim doses every three hours.

26th.—“Slept well last night, complains of great soreness all over, and that he cannot see, cannot recognize a watch, when asked to look towards a person standing at his bedside, his eyes immediately seek the wall behind him where

he says he can see the shadow, has no idea of distance, hears well, constant cough. Temp. 99 fahr., pulse, 104; compressed by 600 grammes of sphygmometer; urine 27 ounces, sp. g. 1015; strongly acid; colour febrile; Albumen, one fifth; 458.6 grains of urea in 24 hours, in the 27 ounces; or 169.8 grains in 100 oz. food, milk; tincture of digitalis increased to thirty minims, every three hours.

28th.—“Passed a good night but vomited once and had a rigor at 6 a.m.; goes to sleep after each dose of the medicine; sees better; feeds himself; urine 69 ounces; 918 grains of urea; contained therein or 153 grains in 10 ounces; Pulse softer, compressed by 230 grammes.

29th.—“Medicine omitted, to be repeated if head symptoms appear again; urine, 36 ounces; sp. g. 1015; contains 550.8 grains of urea; 17 grains of sod. chlor.; 16.4 grains; phos. acid; in 10 ounces of urine—153 grains of urea, 4.7 sod. chlor., 4.5 phos. acid.”

He continued to improve after this; the urine temporarily diminished in quantity and the amount of urea decreased at the same time.

Dec. 2nd.—“Temp. 98; pulse rather harder again; urine 28 ounces; sp. g. 1112; albumen a trace; contains 207 grains of urea, or 74 grains in 10 ounces of urine; 20 grains of sod. chlor., or 7 grains in 10 ounces of urine; 12.7 grains of phos. acid, or 4.5 grains in 100 ounces of urine.

6th.—“Sweating profusely at night, urine, 42 ounces, sp. g. 1012; contains only 200 grains of urea, or 47.8 grains in 10 ounces; headache almost gone, sight much better, can tell time by a watch.

17th.—Commenced to complain again of intense headache, loss of memory etc., and to have bad nights; ordered fifteen and twenty five minim doses of tincture of digitalis and then bromide of potassium, but was only relieved Dec. 20th, when leeches were applied to the temples.

21st.—“Head clearer, slept pretty well feels much better.”

It is interesting to note that on this occasion and on others, after an exacerbation of the nervous symptoms, the amount of urea eliminated in the 24 hours in the urine increased, as if it had previously been accumulating. It reached 405 grains on the 27th, and 770 on the 30th, when 70 ounces of urine were passed in 24 hours with a sp. g. 1017.

Jan. 1st.—“Suffering again from headache left arm and leg powerless and sensation impaired very restless; urea, 320 grains in 24 hours.” Relieved by the application of six leeches to temples.

3rd.—“Urea, 530 grains in 75 ounces of urine, in 24 hours.

5th.—“Urea 800 grains in 77 ounces of urine, in 24 hours; head quite free from ache, no reduplication of 2nd sound at base; pulse compressed by 550 grammes of sphygmometer; retinal mischief increased, both vitrii decidedly turbid.

On Jan. 12th.—He began falling off again. “Suffering from headache nausea etc.” At 6 p.m., “head is now very bad.” From this and other nervous symptoms I feared another cerebral attack so ordered venesection to be performed; 17 ounces of blood were withdrawn. “Patient says that his sight became clearer, that the headache was relieved at once.”

13th.—“Better night than usual, head clear, free from pain.”

14th.—“Improving steadily; urine 70 ounces, 510 grains of urea.

16th.—“He states that he does not now waken with a headache as he formerly did.”

During the whole of this month he progressed favourably; on the 24th, urine 80 ounces in 24 hours; acid sp. g. 1012; contained 918.6 grains of urea, and a trace of albumen. On the 26th, 76 ounces, in 24 hours contained 872.4 grains of urea. These results my clinical clerks and myself frequently confirmed by repeating the analyses, and more than once by Dr. Russell West's method.

Feb. 4th —“Allowed to get up; relapses after this period were less serious and occurred at wider intervals. On account of headache leeches were applied to the temples, February 10th and March 5th, on these occasions the amount of urine had diminished to about one half that usually excreted. He continued the mixture containing iron, and was religiously confined to milk diet. He was soon able to remain out of bed all day long and made himself useful in the ward. He passed something like 55 ounces of urine in 24 hours, which contained about 350 grains of urea, and a mere trace of albumen. He was able to leave the hospital April 13th, 1877.”

I must not further extend my paper—but I may just allude to the case of a man now under my care who was admitted with renal disease and some bronchitis and emphysema. He was attacked several times at night with violent fits of dyspnoea, regarding these as probably uremic in nature, as very little urine was passed and there was nausea, contracted pupils and low temperature I ordered him to be bled to 10 ounces. No further asthmatic paroxysms occurred. A few days afterwards he had an eclamptic attack, a

little more blood was removed by my house physician Mr. Gorst, the patient immediately regained consciousness, and so far is doing well.

Note on the Case of T. D.,—page 130.

October, 1877.—Venesection was practised on two occasions subsequently, to relieve orthopnoea, with the best results. He died October 1st, death being due to exhaustion, from pain and from pulmonary complications. An aneurism of the innominate was found, partly consolidated, pressing on the bifurcation of the trachea; there were cheesy deposits in both lungs. It is an interesting fact, that the amount of urea in the urine was greatly increased after venesection.—On June 25th, two days after venesection, 18 ounces of urine, sp. g. 1035, were passed in 24 hours (the patient was limited to one pint and a half of fluid in 24 hours,) and contained 509 grains of urea. On July 3rd, 16 ounces of urine, sp. g. 1026 were passed in 24 hours and contained only 232.2 grains of urea, the quantity he commonly excreted. This observation appears to support the assertion of Bauer,—that venesection leads to an increased decomposition of albuminous bodies and an increased excretion of urea. Other circumstances dependent on the temporary dyspnoea may have induced this phenomenon, such as increased waste of muscle from violent respiratory efforts, and accumulation of nitrogenous compounds in the blood from imperfect oxygenation.

ON THE PHENOMENON OF SO-CALLED “ DIRECT ” PARALYSIS.*

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AT no period perhaps in the history of the Healing Art have the advances in the domain of Physiology and Pathology been so rapid and so great as they have been during the last few years and continue to be at the present moment. In no one of the numerous special departments of the animal economy have these advances been so well marked as in that which relates to the *Nervous System*; and nowhere does so extensive and important a field lie still open and unexplored to exercise the patience and skill and stimulate the energy of the anxious enquirer after scientific truths.

Physiology and Pathology have at all times been cognate and intimately associated sciences, and in the mind of the practical

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\* The present paper in its original form was written, but never published, upwards of a year ago. Since then the subject of “ Direct Paralysis ” has been brought prominently before the Profession by Dr. Brown-Séquard in his various public lectures and articles contributed to the Medical Journals. As well, therefore, to economize space as to avoid repetition of facts and arguments, considerable abridgement and alterations have been necessitated in this place. The details of a series of experimental vivisections, undertaken in connection with the above subject but not as yet completed, are also here omitted—*E. H. D.*—*Ap.* 1877.

physician they are necessarily inseparable. But in no case perhaps is the closeness of their relationship so especially exemplified as in connection with their bearings upon the Nervous System. Between the normal and morbid processes of Innervation it is indeed often extremely difficult—at times well nigh impossible—to draw a border line; the gradations between them are in many cases so delicate, the distinctions so subtle and refined. And whereas, without an accurate knowledge and appreciation of the functions and conditions of health, it is futile to attempt to understand and explain the varied phenomena of disease; so also, on the other hand, do we constantly obtain from correct and intelligent observation of these latter the clearest possible proofs and the most valuable confirmatory illustrations of numerous physiological data, which would otherwise remain vague and indeterminate, often the results of mere speculative inference or—in so far as they have relation to man—analogue deductions from experiments upon the lower animals.

As has been said, great strides have indeed been made of late in all that appertains to our knowledge of the Nervous System and especially of its great centre—the Brain. In proof of this we need only here refer to the remarkable and important results recently arrived at by Fritsch and Hitzig, Ferrier, and others in regard to the localization of the functions of the cerebral hemispheres, results which, as has been justly remarked, are of themselves sufficient to mark an epoch in the history of neurology. These researches, and such as these, have opened out to us a vast field of information and instruction and have laid the foundations of what we may almost regard as a new branch of scientific knowledge, as yet indeed only in its infancy and comparatively imperfect in its details, but destined in all probability at no very distant period to rank as irrefutable and exact; and, as such, to prove of invaluable assistance to us in our endeavours thoroughly to comprehend and in some cases effectually to regulate the mechanism of the human frame. But while much good work has been done and much insight acquired into the physiology and

pathology of the Brain, our knowledge of the subject is, it must be confessed, as yet very crude and insufficient. We are inclined to grasp eagerly at the apparent results of observation and experiment and from these to deduce conclusions and establish doctrines which may indeed be, in the majority of cases, perfectly logical and correct. Every now and then, however, startling facts, utterly anomalous and often wholly inexplicable in character but stubborn and incontrovertible in their reality, stare us suddenly in the face, breaking rudely down—it may be—some of our most cherished theories and shaking the validity of principles which we have long regarded as axiomatic and infallible.

Of all the strange anomalies which from to time come before our notice in connection with cerebral pathology none perhaps are more remarkable and difficult of explanation than the cases of so-called "*Direct Paralysis*" the term "Direct" (borrowed from the French) being here used—in contradistinction to the "Crossed," "Diagonal," or more ordinary form—as tersely and conveniently denoting the occurrence of a hemiplegia on the same side of the body as the seat of its causation in the brain.

These cases present features of so peculiar and unorthodox a type, and appear so much at variance with every day clinical and pathological experience and almost universally acknowledged physiological laws, that they are, to quote the words of Dr. Brown-Séquard, "in the actual state of science looked upon with very dubious eyes." Indeed there are doubtless many who still refuse to recognize the very existence of the phenomenon of Direct Paralysis, and who believe that it is only through some inaccuracy or omission on the part of the observer that the supposed instances of it find a place at all in the annals of pathology or are allowed as facts, however inexplicable, by men of practical and scientific knowledge. Nor can we wonder that this should be so: traditional and old-standing beliefs are not easily uprooted nor are men readily disposed to the reception of new doctrines based upon isolated and comparatively rare phenomena, examples of which they may never have had an opportunity of seeing for themselves

and of which it may be difficult and often impossible to give a definite and categorical interpretation.

Nevertheless that cases of the class above alluded to do occasionally occur (and perhaps more frequently than is generally supposed) is surely beyond question,—a very considerable number of such having been from time recorded by men of whose scientific competency as well as truthfulness and accuracy of observation there can exist no manner of doubt.

It is indeed only a matter of wonder, as it seems to me, that the subject of Direct Paralysis, being one of such interest and importance and apparently so well established by authoritative evidence, should not have attracted more general attention and merited more careful and extensive investigation at the hands of modern pathologists. Very many of them have, it is true, noticed and described at length instances of this affection; but comparatively few (among whom may be mentioned Dechambre, Hilton, Bayle, Nasse, and notably Brown-Séquard,) have devoted any especial attention to the subject or have sought to reconcile its occurrence with the principles of scientific and correct physiology. Some of the most able and voluminous writers upon Medicine and Pathology of recent times have either altogether ignored the existence of Direct Paralysis, or have merely alluded to it in the most cursory manner. This may be in part due to certain doubts and misgivings as to the true nature or accurate representation of the cases which have been recorded: in part—to the innate and not unjustifiable conservatism, already referred to, which refuses to relinquish old faiths unless the conflicting evidence against such be of the most positive and incontestable character: but most of all is this reticence due, I apprehend, to the difficulty of affording a clear and satisfactory exposition of so unusual and anomalous a phenomenon.

As an example of what I have first said I may quote the words of the eminent teacher and clinical observer, Prof. Andral, who, in treating of “Diseases of the *Encephalon*,”\* dismisses the subject of

\* *Clinique Medicale* par G. Andral.

Direct Paralysis in a most summary manner—"It may therefore," he writes, "be laid down as a law that the paralysis affects the side of the body opposite to the hemisphere wherein the effusion of blood has taken place. The cases in which the contrary has been observed are so few in number that they can be considered but as very rare exceptions to the law just mentioned, *which exceptions again have not yet been accounted for.*" [The italics are my own but the words are of themselves sufficiently expressive.]

Again—one of the most earnest scientific and clinical workers of the present day, Dr. Charlton Bastian, in his recent admirable little treatise on "*Paralysis from Brain Disease*" (Lond. 1875) does not hesitate to acknowledge the undoubted existence of the "direct" forms of hemiplegia; for, he says, "we are unable to explain away such instances by supposing that mistakes have been made, in substituting 'right' for 'left' or vice versa, in the writing out or dictation of post-mortem records --though perhaps some of them may be thus accounted for." Beyond this, however, Dr. Bastian scarcely ventures, contenting himself with the simple confession that "The occurrence of paralysis or of convulsion on the same side as the brain lesion is with our present state of knowledge quite inexplicable;" and adding—"still more mysterious, therefore, is it when we find one hemisphere apparently more apt than the other to produce such an anomaly." This last sentence has reference to the fact first pointed out upwards of forty years ago by Bayle and Dechambre\* and since corroborated by Brown-Séquard and others—that unilateral *direct* paralysis is much more frequently associated with lesions of the *right* than with those of the left half of the brain.

Another eminent observer, the late Prof. John Hughes Bennett, alluding to this subject, advances as a probable explanation of it that—"as paralysis may be induced without leaving any traces, in these few cases (of direct hemiplegia) it was caused by unknown changes in the opposite hemisphere of the brain: and as is sometimes the case, the lesion found in the hemisphere of the

\* *Bulletin Clinique*, Paris, 1835. p. 116.

paralysed side had produced no effect."\*—This theory might indeed serve to explain some few cases; but for a far greater number it could scarcely be accepted as true, and it is, under any circumstances, altogether vague and inconclusive.

But of all those who have from time to time, at the present day and in the past, made reference to the existence of Direct Hemiplegia, there is no one to whom we are so much indebted for a careful and elaborate investigation into this interesting phenomenon as we are to Dr. Brown-Séquard. Ever since that able and enthusiastic physiologist first directed the attention of the medical profession in 1858 † to this particular feature in cerebral pathology he would appear to have made it a matter of special study; until by personal observation of several well-marked cases, by careful investigation and analysis of a large number seen and recorded by others, and by repeated confirmatory experiments upon the lower animals, he has been enabled to arrive at some very interesting—and, as he himself confidently believes, very definite and satisfactory—results. Many of these results he has at intervals communicated to the scientific world and has concisely summed up and reviewed the essential points of his researches upon this subject in a series of very instructive papers published in the *Lancet* at the commencement of last year; ‡ and, more recently still, in several courses of Lectures delivered publicly at the Royal College of Physicians in London, and elsewhere, and duly reported from time to time in the various Medical Journals. To the nature and value of his opinions and the conclusions at which he arrives we shall have occasion to make reference later on.

It has been already stated that a very large number of cases of Direct Paralysis are on record, and Brown-Séquard tells us that

\* *Principles and Practice of Medicine*, 1858.

† *Journal de la Physiologie de l'Homme et des Animaux*. Vol. I. p. 523—38, et 755—83. Also Lectures on "*The Physiology and Pathology of the Central Nervous System*," delivered at the Roy. Coll. of Surgeons, London, in May, 1858.

‡ *Lecture on the Appearance of Paralysis on the side of a lesion in the Brain*. *Lancet*—January 1st, 15th and 29th, 1876.

he has himself collected, "more than two hundred." A large proportion of these have been very carefully described and many are thoroughly well-authenticated: some of them also date back for a very considerable period, though this may in some degree, it is true, throw a doubt upon their absolute pathological significance owing to the limited facilities for minute observation then at command. That the occurrence of this class of cases has been long recognized we may gather from the fact of their having been specially noted by Morgagni in his "*Epistola Anatomica*,"\* and in his great work "*De Sedibus et Causis Morborum per Anatomen indagatis*," which was published in 1761, the author being then in his eightieth year. The attention of Morgagni was particularly likely to be attracted to this form of pathological anomaly, inasmuch as his own teacher, the illustrious Antonio Valsalva, was the first to demonstrate with any degree of clearness and to establish the general law of "crossed paralysis," viz.,—"that the internal cause of the hemi-plexia is not on the same side with the disease, but on the opposite." "I have already pointed out," says Morgagni, "some who saw this before Valsalva but neglected it as accidental, as Wepfer and Baglivi."† Morgagni, however, was of opinion that Aretæus was acquainted with the principle of "crossed paralysis;"‡ as indeed one has some right to infer from a passage of the latter author's, relative to the treatment of cerebral apoplexy, where he says "we must consider whether the paralysis be on the left side or the right for we must take blood from the healthy parts, whence the blood flows more freely, and a derivation is made from the affected parts," i.e., the injured hemisphere, which, it is seemingly implied, corresponds to the sound side of the body.

Morgagni describes at length the case of an old woman who

\* *Epist. Anat.* xiii. n. 25.

† *De Sed. et Caus. Morb.* Lib. i. Ep. 3. 18.

‡ N.B.—The "crossed paralysis" spoken of here and elsewhere in this paper is not to be confounded with the "cross paralysis" or "alternate hemiplegia" of Romberg and Gubler.

had an attack of apoplexy succeeded by paralysis of both limbs on the right side, though not quite complete and with sensibility unimpaired. About three months after the seizure the woman died, and Morgagni made a very careful P. M. examination and dissection, which, as he tells us, occupied him "several days." After noting fully the condition of the right half of the brain where extensive softening existed, he adds, "In the whole left hemisphere of the cerebrum, contrary to what the hemiplegia had given us reason to expect, no disorder was found." Commenting further upon the peculiarity of this case, Morgagni says, "Moreover, a paralysis of the body on that side which lies beneath the injury of the brain, instead of being opposite to it, is not a very common thing. You may, therefore, add this observation to the few others which I have referred to heretofore,\* among the much greater number of opposite paralyses, as if by way of some exceptions." In connection with the above, Morgagni also refers to a case observed and recorded by one Carlo Curti, a Neapolitan physician, in which "to a resolution of the right side, in consequence of an apoplexy, he saw a correspondent corruption of the substance of the cerebrum, *not in the left but in the right hemisphere*; and that in the whole of it, so that even the meninges were converted into a mucous substance."†

I have thus alluded to the cases spoken of by Morgagni, for they possess, it seems to me, great interest (if not indeed positive scientific value,) as having attracted attention and been deemed worthy of special record at a period when cerebral pathology was comparatively in its infancy.

It is, however, no part of my intention in this place, even if space permitted, to enumerate a host of instances or enter upon a description of their details in order to prove or illustrate the occurrence of true Direct Paralysis. The onus of this task has been already undertaken and very fully and ably discharged by Dr. Brown-Séquard and others better versed than I am in the literature of nervous

\* *Ep. Anat.* xiii. n. 25.

† *De Sed. et Caus. Morb.* Lib. iv. *Ep.* lviii. 15.

pathology. I shall be content, therefore, with reference to very few of such cases, restricting myself mainly to the careful narration of one which about a year ago came under my own immediate notice and which first directed my attention to the subject of the present paper. The case occurred in my Hospital practice and was from the first one of some interest though it presented clinically no symptoms which could be regarded as unusual or anomalous. It was only at the P. M. examination that its peculiar characteristics were revealed and these were assuredly not a little perplexing. It was the first and only instance of the kind I had come across nor was I at the time, I must confess, at all well acquainted with the history of this class of pathological phenomena. Anxious to find some record of anomalies similar to that which I had just witnessed, I proceeded, on my return from the Hospital, to search diligently the pages of such works as I happened to have by me at the time likely to throw light upon the subject of my enquiry. In one of the first of these that I took up, Bright's "*Reports of Medical cases*," [Vol. ii. p. 306-8,] I found an illustration to the point, which I think is worthy of brief quotation in this place.

The case is that of a man who died in Guy's Hospital in 1829. He was admitted suffering from partial left hemiplegia, which was complete at the time of seizure twelve months previously. The P. M. examination shewed a yellowish opaque induration of the brain substance, with an old apoplectic cyst, on the *left* side. "The whole of the remaining parts of the brain, and more particularly those of the right side, were most carefully examined, but without finding the slightest trace of disease." Dr. Bright, commenting upon this case, remarks that "Its great peculiarity was the occurrence of paralysis on the same side as the lesion of the brain; and this required all the evidence of relations and friends, confirmed by the observations made by the medical attendants during the short period he lived in the Hospital, to induce even those who had seen it to feel positive of the fact." Dr. Bright, however, makes no attempt to explain the anomaly.

Within a day or two after the occurrence of my own case of

Direct Paralysis, and whilst still searching for further examples and some satisfactory explanation of such an appearance, my attention was drawn to the article in the *Lancet*,\* before referred to and already published in part a few days previously, but which I had not myself happened to notice. From this I obtained much useful and instructive information in reference to Direct Hemiplegia, and since then have endeavoured to make myself more thoroughly conversant with the literature of this interesting subject. A large proportion, however, of such literature has been published in Germany and France, especially the latter, and very frequently in journals, separate papers, and treatises, not usually found in Medical Libraries in this country, and to several of which I have been unable to obtain access.

Before proceeding to the relation of the case observed by myself, I may just briefly refer to one or two others; and first and more especially to one, the particulars of which were kindly communicated to me about the time I am speaking of by my friend Dr. W. Williams, as having come under his notice some three years ago at the Royal Southern Hospital, in this town.

"A man was admitted into the Hospital with symptoms of severe concussion and with a compound and depressed fracture in the *left* side of the skull. Complete return of consciousness took place on the 3rd or 4th day, and general recovery from the worst symptoms (extending over a period of some months) also followed to a certain extent, though he was still affected by any noise and subject to a slight though constant and stationary pain in the head over the seat of the injury. Ultimately Epilepsy, from which the patient had never previously suffered, manifested itself, a distinct aura travelling up the left arm to the face and head on the same side. Coincident with the inroad of the epileptic attacks, loss of power was noted of the *left* arm and leg, gradually increasing and eventually producing some wasting of these extremities. The skull was subsequently trephined over the original seat of injury, and after removal of a sharp spiculum of bone which projected obliquely inwards for about one-eighth of an inch and impinged upon (though without lacerating) the dura mater, total recovery took place, the epileptic symptoms being the last to vanish. The

\* Lecture by Brown-Séquard, *Lancet*, Jan. 1st, 15th, 29th.

operation was performed nearly three years ago, and Dr. Williams tells me that he has quite lately seen the man, who is perfectly well and working regularly at his business."

Such are, I think the main points worthy of note in the history of this very remarkable and thoroughly well authenticated case.\*

In addition to the above, the particulars—more or less complete—of two or three other unpublished cases of apparent Direct Paralysis have been kindly forwarded to me, with permission to make use of them, by professional friends who have known that I was interested in this subject. I shall, however, only here refer—and that very shortly—to one of these, some notes of which I have recently received from Dr. Cavafy of St. George's Hospital, London. I select this case not because it is in all respects a very typical one but because it possesses one or two features of special interest in connection with observations previously recorded by others.

"The case is that of a woman, aged 62, who came under Dr. Cavafy's care in August, 1876. The family and antecedent personal histories were good: the catamenia continued to the age of 55. The patient stated that during the previous winter she had been a good deal exposed to cold and had frequently got wet in the feet. One morning in June, on awaking in the morning, she found the *right* side of the face and both hands partly paralysed, with a dark semi-opaque spot apparently moving about in front of the *right* eye. The paralysis on the right side of the face gradually grew more complete, while in the hands it ebbed and flowed. The black spot disappeared in about a week, but the vision of the right eye subsequently remained less clear than that of the left.

Her condition when she first came under treatment was briefly as follows:—There was partial paralysis of the levator palpebræ and complete of the other facial muscles of the right side from the occipito-frontalis downwards. The pupils were equal in full light but the left dilated more readily than the right. The tongue was protruded to the right side. There was considerable loss of power in both arms and hands—most so in the right, and less of flexion and extension than of pronation and supination. Lower extremities comparatively

\* Since the above was written I have found that Dr. Williams' case has been published at length in the '*British Medical Journal*,' [Nov. 4th, 1876, p. 586.] It is, however, one of such great clinical value and importance that I make no apology for thus recapitulating its chief features. The full report will well repay perusal by any one who takes an interest in the subject of the present paper.

unaffected, only the right being slightly so. There was great thirst and anorexia. Tongue red, dry, and fissured. Temperature, 97·8. Pulse—strong, 106. Urine, scanty, rather turbid, and albuminous. The patient became rapidly worse: some inflammation set in about the right angle of the jaw: the general weakness increased and the breathing became feeble and oppressed: blood began to be passed with the motions, and she died early in September.”

“*P. M. Examination*, fourteen hours after death.—Little worthy of notice in general condition of the viscera except that the heart was considerably hypertrophied, weighing 15 ounces. [Kidneys—granular?] Right parotid gland and neighbouring cervical chain enlarged and congested. Brain—somewhat anæmic and arachnoid containing a little serous fluid. General cerebral substance otherwise healthy except a portion of the inner and under surface of the middle cerebral lobe *on the right side* which was found adherent by fine filaments of tissue to the fundus of the middle fossa of the base of the skull. At this spot, on removal of the superjacent brain, the dura mater was found to be diseased, and there was then seen a round, flat, and irregular growth, about the size of a shilling, partly situated upon and slightly projecting from the right posterior surface of the body of the sphenoid and adjoining portion of the petrous bone.”

[The notes I possess of the case generally, and especially of the P.M. examination, are somewhat meagre, but the latter are supplemented by a drawing which, if space had permitted of its introduction here, would have considerably facilitated a description of the exact position of the tumour and its relation to surrounding parts. It may, however, be pretty accurately stated to have chiefly occupied the apex of the petrous portion of the temporal bone, rather behind and to the outside of the posterior clinoid process of the sphenoid on the right side; so that it would impinge upon the inferior surface of the middle cerebellar peduncle on that side, along with the apparent root of the trigeminal nerve. As to the growth itself it is described as being “mammillated on the surface and to the naked eye like a papilloma”. Its situation would however probably negative this supposition as to its pathological characteristics and unfortunately no microscopic examination appears to have been made.]

Now I have not, as I before said, introduced this case as an absolutely typical example of a Direct Paralysis; for, though its

main features place it undoubtedly within this category, yet the loss of muscular power was (except in the face) more or less limited and varying in intensity and, moreover, extended in some degree to the left as well as to the right hand. This latter fact is of itself of some interest, seeing that the brain lesion—which we must reasonably believe to have been the cause of the paralysis—was entirely confined to the right hemisphere, and that throughout the whole extent of the cerebral substance nothing abnormal was found elsewhere.

That the fifth nerve was partially implicated would seem to be evidenced by the stiffness and inflammation about the angle of the lower jaw, through pressure upon or irritation of the lesser or motor branch supplying the temporal, pterygoid, and masseter muscles. The partial affection of the vision in the right eye is also a point of some physiological interest.

The most noticeable feature however in the case just related is in connection with the particular situation of the brain lesion. As far back as 1858, in a paper published by Dr. Brown-Séquard and already referred to, entitled "*Recherches sur la Physiologie et la Pathologie de la Protuberance Annulaire*,"\* as well as in a course of lectures on "*The Physiology and Pathology of the Central Nervous System*," delivered by him in the same year at the Royal College of Surgeons in London, that physiologist pointed out that although a "Direct" Paralysis every now and then occurred as a sequence of a lesion situated in very varied regions of the brain, yet that there were certain limited areas, chiefly about the base, injury to or disease of which was especially apt to give rise to this phenomenon. Of such special areas the principal appear to be the *crura cerebelli* and parts situated immediately over the petrous bone. In one place Brown-Séquard says "Indeed in cases of disease of the petrous bone or of the surface of the encephalon near that bone, paralysis, if it occurs at all, will appear as frequently on the side of the lesion as on the opposite side." And again, he says, "When a tumour exists, pressing upon the

\* *Journal de la Phys. de l'Homme et des Animaux*, p. 523.

anterior surface of one of the crura cerebelli and the insertion of the trigeminal nerve, if it causes paralysis, it is in the same side of the body. I have collected fourteen cases of this kind, all having the same features; *incomplete* paralysis in the side of the lesion, no anæsthesia (except in one case), and frequent fits of vertigo." \* Now it will be at once seen that these statements have a very distinct bearing upon the case I have last narrated. I have to regret that my brief notes give no data as to the presence or otherwise of anæsthesia or of vertigo, but certainly in this particular instance the nature of the paralysis and the seat of the cerebral lesion bear a very precise analogy and afford a striking exemplification of the proposition laid down by Dr. Brown-Séquard.

Into the explanation offered by this observer for the occurrence of the above special class of cases I shall not at present enter, but shall at once proceed to give the details of the example of Direct Paralysis which has come under my own immediate notice.

Mary O'Neill, æt. 33, single, was admitted into the Liverpool Northern Hospital under my care at 11.30 p.m., on January 3rd, 1876, in a state of apparent unconsciousness, in which condition she had been discovered lying under an archway in a neighbouring street. She was at once seen and attended to by the House-Physician, Dr. Craigmile, to whom I am indebted for the careful observations made and recorded at the time of admission, before I had the opportunity of seeing the case for myself, as well as from time to time during its progress.

It was subsequently ascertained that the woman was of loose character; that she had been drinking rather heavily during the day; and that she had gone out with a companion about 9 p.m. to procure some more drink. How she came to be in the position in which she was found there was no evidence to shew, nor could she herself remember afterwards anything that had occurred that night.

*On Admission.*—The patient was seemingly in a state of coma: the breathing was slow and laboured but not stertorous. There was no convulsion, foaming at the mouth, puffing out of the cheeks, nor deviation of the features. The left pupil was widely dilated: the right (if anything) very slightly contracted; both of them being insensible to light, as were also the corneæ to touch. Heart sounds normal: pulse regular, but rather weak. There was no sign of any fracture, contusion, or blow on the head or body. Seeing that there was a strong

\* *Lancet*, Dec. 25, 1858, p. 652.

smell of alcohol about the woman, the stomach pump was employed by the House-physician but scarcely anything was evacuated by its means. While the stomach pump was being used the first return to anything like consciousness was manifested, in the shape of sundry pretty vigorous efforts to seize hold of and put aside the instrument; and it was then specially noted that in such efforts the *right* hand only was used. On further pinching and pricking the surface of the body, the whole of the right side responded pretty freely, but the *left* little if at all, only the slightest motion of the finger ends being perceptible.

On the following morning (January 4th), when I first saw the patient, I found the pupils now equal and normal in size and acting fairly under the stimulus of light. She could then move the left arm and leg slightly, though there seemed to be great reluctance and tardiness in doing so, and there was evidently considerable loss of power, with apparently somewhat diminished sensibility and reflex action, on that side of the body. The functions of the *right* side were apparently perfect. There was complete ptosis of the *left* eyelid with external strabismus, distinctly marking paralysis of the left 3rd nerve. The tongue was protruded straight, and the patient was rational and able to articulate fairly, and could even give some little account of herself when roused and pressed for a reply to questions. There was great drowsiness but no spasm, pain, nor vomiting, and during the day she managed to take some milk and beef tea by the mouth.

January 5th.—Little alteration in the symptoms, but patient seemed rather less heavy and stupid. It was also found that during the night she had got up before the nurse could prevent her and had used the bed-stool. This she again did in the morning, the bowels and kidneys acting naturally. The urine was normal and contained no trace of albumen nor any deposit. Later on, the patient was able to walk across the ward, though she dragged the left leg a little and the left arm hung passively at her side. She could, however, use the latter to a slight extent when urged, but there was still a very marked diminution of power in the limb.

This remained much the condition of things from the 5th up to the 10th inst., the patient usually lying dozing in bed the whole day and requiring to be roused at intervals to take nourishment or answer questions. The paralysis of the left 3rd nerve continued, but the pupils were equal in size and normal in action.

January 11th.—By the morning of this day the woman had apparently made some real progress towards recovery, and, on a friend of hers having called at the Hospital and brought her some clean linen, she got up and dressed herself, insisting upon going home at once. She had been sitting up for an hour or so, when, on rising from her chair rather sharply, she suddenly became very stupid and drowsy and had to be again lifted into bed.

Coma rapidly set in : the urine and fœces were passed under her, and she was fed by brandy and beef tea enemata and by the stomach pump. There was no epileptiform convulsion nor facial distortion : the pupils were of equal and normal size but not acting at all to light. There still remained some slight reflex action and muscular response to pricking and pinching over the surface of the body *except on the left side*, the insensibility of which seemed now complete, or, at any rate, there was a total loss of motor power in the limbs. The patient continued in this state until 7.30 p.m., when stertorous breathing (hitherto absent) commenced and she died shortly before midnight, ten days after the first—and some sixty hours after what may be regarded as the second—apoplectic seizure.

The above are, I think, the main features which occur to me as being worthy of record during the progress of the case. Fortunately the notes taken of it were pretty full ; though possibly, had we been able to diagnose at the time the exact character—or at least situation—of the cerebral lesion as revealed after death, there might have been still more careful and extended observations made of minutiae and of the less prominent symptoms which presented themselves for clinical investigation.

*Autopsy*—Sixteen hours after death. Body well formed and nourished. No fracture, contusion, nor other evidence of external injury. On removal of the calvarium and opening into the dura mater (which was healthy and entire), a considerable quantity of extravasated blood, amounting perhaps altogether to about 3 ounces, was found spread out in a layer over most part of the lateral surface of the *left* hemisphere, and lying in the cavity of the arachnoid. The extravasation was *entirely* confined to the left side and extended downwards along the Sylvian fissure to the base of the brain where a small clot, the size of an ordinary horse-bean, was seen pressing upon the trunk of the left 3rd nerve. The blood effused was for the most part dark, clotted, and firm, and presented a somewhat shrunken and honey-combed appearance, as if it had existed there for a good many days. About the centre, however, of the left parietal lobe there was quite a fresh-looking and semi-fluid clot, of about an ounce in bulk, lying more superficially upon what appeared to be the older extravasation and gradually tailing off in quantity down the fissure of Sylvius. No rupture of the arachnoid could at first sight be discovered, nor any special focus from which the hæmorrhage might have sprung.

Afterwards, however, on careful removal of the clot and a more minute examination of the brain surface, the point of origin of the bleeding was clearly exposed. This had evidently arisen from the rupture of a small branch of the middle

cerebral artery situated in the left intra-parietal fissure. The coats of the vessel seemed perfectly healthy, but, at a point about  $2\frac{1}{2}$  inches downwards from the great longitudinal fissure, they were torn half through and presented much the appearance of having been lacerated by a blow. The lateral parietal convolutions of the left hemisphere were slightly flattened and their surface deeply stained with blood where the clot had pressed upon them. Nowhere, however, was there any irruption into their substance nor disturbance of the pia-mater and arachnoid except at the bleeding point above indicated. The ventricles were opened into and in the right one only was found any fluid, amounting to about a drachm and a half of clear serum. The whole of the right cerebral hemisphere, both its superficial and internal structure, was most minutely and accurately examined (portions of it microscopically); but, with the exception of the small quantity of fluid in the ventricle, no lesion nor abnormality whatever was discernible; nor had the effused blood come into contact at the base, nor at any other point, with the surface of this side of the brain. The vessels of the right hemisphere were dissected out and closely observed; their coats were perfectly natural and nothing like embolism or thrombosis could be found. The Crura Cerebri, Pons Varolii, Cerebellum, Medulla Oblongata, and upper portion of the spinal cord were also very carefully examined but without revealing anything of an unusual character.

[N.B.—It may be here specially noted that the decussation of the anterior pyramids of the Medulla was very well marked in the fresh state; and, the parts having been hardened and afterwards examined microscopically, exhibited no appreciable deviation in the crossing and arrangement of the fibres from the appearances universally recognized as normal.]

Finally—all the other organs of the body were found to be perfectly healthy.

Before proceeding to discuss or venturing to offer any explanation of the apparent anomalies in the history of the case just detailed, it perhaps devolves upon me, in the first place, to demonstrate as clearly as possible the actual existence of such anomalies. I shall, therefore, endeavour to anticipate and answer some of the more probable arguments which might be adduced to shew that the case before our notice is not really to be regarded as one of 'Direct' but rather of ordinary Crossed Paralysis, and that, although the symptoms during life were, in relation to the post-mortem appearances, unusual and at first sight perplexing, they were nevertheless capable of what we may call the orthodox or commonly received explanation.

In examining the particulars of this case, we are led first of all to enquire what was the initial cause of the hæmorrhage into the arachnoid, or at least of the rupture of the vessel from which such hæmorrhage arose?

Seeing that in the adult, where (as in the present instance) there exists no aneurismal or other apparently morbid condition of the circulatory apparatus, the most frequent cause of meningeal apoplexy is external injury; and, looking at the position in which the woman was found after a heavy drinking-bout,—it seemed at first sight not an improbable supposition that the rupture of the vessel was due to concussion from a fall whilst she was in a state of drunkenness. No evidence, however, of any such fall or blow upon the head was found, and it is, I think, equally possible that the vessels of the pia-mater, being in a state of engorgement and abnormal tension from the stimulating effects of alcohol,—one of them suddenly gave way independently of any immediate external agency.\*

That the arterial coats should have proved to be affected and altered by *syphilitic* disease I was not unprepared to find, the woman having for some years led the life of a common prostitute; indeed before death, and in the absence of any other apparent cause, I was somewhat inclined to attribute the symptoms noticed mainly to the effects of such specific and morbid action upon the cerebral vessels. The post-mortem examination, however, entirely failed to support such a supposition: but whatever be the true explanation of the hæmorrhage, it does not materially effect the more important physiological aspects of the case.

Now we have here a well-marked lesion on the left side of the brain and coincident therewith a distinct—if only partial—hemiplegia on the same side of the body. That the latter was (in the technical sense of the word) the *direct*—as it was evidently

\* A case of an almost identical character, which was brought into St. George's Hospital some years ago and was at the time of much medico-legal interest, is mentioned by Mr. Holmes in his '*Treatise on Surgery—its principles and practice*.'—1875, p. 136.

the *immediate*—result of the lesion aforesaid it will be my object to shew, nor do I apprehend that there will be much difficulty in accomplishing this in a clear and satisfactory manner.

The hemiplegia was, it would seem, almost complete when the patient was first brought into the Hospital, though it disappeared afterwards to a considerable extent; a circumstance probably in the main attributable to relief of the pressure upon motor centres, caused by the partial absorption of the serous portion of the clot, and to the gradual and compensatory accommodation of the brain substance generally to the disturbed equilibrium of the cranial contents. The secondary and fatal seizure was without doubt owing to a further effusion from the ruptured vessel, consequent upon the patient's exertions in dressing and moving about. This was clearly shewn by the post-mortem appearances, the recent clot being readily distinguishable from that of an earlier date. The fact of this secondary apoplexy having been accompanied by a renewal of the paralysis in all its intensity upon the same (left) side seems to me of itself to demonstrate pretty definitely the so-called "direct" character of such paralysis,—i.e., that a hemiplegia was produced on the same side as the exciting intracranial lesion.

Moreover,—if it be granted that the hæmorrhage on the *left* side of the brain was sufficient to give rise to a paralysis, and it be urged on the other hand that there was some lesion, not apparent, on the *right* side sufficient also to give rise to a paralysis (crossed), should we not be justified in expecting to find the right side of the body affected as well as the left, or in other words that the paralysis would have been general?

In a critical consideration of the present case there is a feature in its history which may perhaps be thought to demand some notice; though, from its very uncertain and comparatively insignificant character, it can scarcely be said to possess any real pathological importance. It has been stated that after death the presence was noted in the *right* ventricle of a small quantity of clear fluid (about  $1\frac{1}{2}$  or 2 drachms at most); but this fact cannot, I

think, be regarded as having any practical value in the relation of cause to effect. The clinical symptoms during life in connection with the post-mortem appearances are, as we know, so extremely anomalous and uncertain in many cases of simple (non-hæmorrhagic) ventricular effusion, even when considerable, that it seems quite impossible to attach any definite significance to the presence of fluid when it exists in so limited an amount. In one case (that of Hahn) quoted by Brown-Séquard there was more fluid in the lateral ventricle on the side of the paralysis than in the other, and in other recorded cases hemiplegia has been found to co-exist with disease of the lateral ventricle on the same side of the brain.\*

There is another point which might possibly be raised to shew that the hemiplegia in the present case was in reality an ordinary one: viz., that, if we suppose the hæmorrhage to have been due to a fall or blow, this might have been upon the *right* side of the head and that the extravasation upon the opposite side was the result of *contre-coup*; in which case there might doubtless be considerable irritation of nervous centres, with disturbance of their functions, in the immediate neighbourhood of the impact. That cases distinctly of this description do not unfrequently occur is well known; others again may seem strongly to encourage such a supposition as to their true pathological character but at the same time lie more or less open to doubt, and often present considerable difficulties as to their accurate diagnosis.

As an example of the latter class I may just briefly allude to an interesting case of "*Left-sided Hemiplegia following an injury to the left side of the Head*," which was brought before a meeting of the Northumberland and Durham Medical Society, in Nov. 1875, by my friend Dr. Byrom Bramwell of Newcastle.† The patient (who was exhibited to the members of the Society) had about a year previously fallen backwards over a distance of eight feet, striking the *left* parietal eminence against a projecting piece of

\* *Lancet*, January 29th, 1876, p. 160.

† *Brit. Med. Journ.*, Jan. 8, 1876, p. 58.

metal. He was insensible for three days after the accident and, when he came to himself, he found the *left* arm, leg, and left side of the face completely useless. He had gradually improved under treatment. Very slight paralysis of the face remained, though the left arm and leg were still markedly affected. The optic discs were perfectly healthy.

Now in this instance it is quite possible, it appears indeed most probable, that the explanation of the symptoms suggested at the time by Dr. Bramwell was the correct one, *viz.*, that "from the long continuance and absolute character of the paralysis, the lesion must have been a laceration of the nervous tissue by *contrecoup*," and that "the lesion was evidently on the right side above the decussation." I think, however, that, in the absence of any post-mortem or other definite evidence we are scarcely called upon to endorse unconditionally such an assertion; and, although we should certainly hesitate to classify the case as it stands as one of "Direct" Paralysis, yet its real nature affords some opening for conjecture, and in several respects, such as the kind and situation of the cranial injury, it very much resembles the example recorded by Dr. Williams and already referred to, the "direct" character of which can scarcely be called in question.

But to return to the case with which we are more especially concerned:—I would say, with regard to it, that the view above broached is inapplicable, or must be at any rate purely problematical. There was no external mark or contusion, and the most careful examination (even with the microscope) of the whole interior substance of the right hemisphere revealed nothing abnormal. Assuredly, then, we have no right in such a case to infer any definite results from the supposed presence of a wholly indiscernible lesion.

Neither can we legitimately assign as a probable cause of the left hemiplegia such a condition as that of *Hyperæmia*, *Anæmia*, or *Œdema* of the *right* cerebral hemisphere, the result of pressure or some similar agency, as proposed by Ambrosi and others, unless we possess at least some appreciable evidence to justify such an

assumption. In the present instance this evidence was altogether wanting.

I would wish also to lay particular stress upon *the healthy condition of the cerebral vessels* on the *right* side, for instances have not unfrequently occurred in which a hemiplegia has appeared at first sight to be caused by a tumour, abscess, or other prominent lesion, found after death in the corresponding half of the brain; whereas further examination has shewn that the paralytic symptoms were—or, at any rate, might be—in reality due to an embolism or thrombosis situated upon the opposite side. Two such cases, for example, are recorded by Dr. W. H. Dickinson in the *St. Georges's Hospital Reports* [Vol. i. 1866, p. 258-9.]

Then with regard to the *position* of the morbid agency in the present instance:—the fact of its being a meningeal rather than a true cerebral hæmorrhage would seem of little material consequence in relation to the effect actually produced; and certainly, whether the extravasation be upon the surface—causing only pressure or irritation, or into the substance of the brain—causing actual destruction, if a similar or equally formidable train of symptoms is set up, surely there can be no reason why the two forms of lesion should not be regarded as having *practically* an equal clinical and pathological importance!

It would indeed appear from the evidence of recorded cases that lesions the most varied, both in kind and situation, within the cranium are capable on occasion (though such occasions be comparatively very rare) of giving rise to paralysis—partial or complete—of the corresponding half of the body. Dr. Brown-Séquard has assured us, as the result of his researches and analysis of a great number of cases, that this appearance may be due to disease (a wound, tumour, abscess, softening, hæmorrhage, &c.,) “almost anywhere in the brain, *e.g.*, the meninges, convolutions, crura cerebri, corpus striatum,” and so forth.

Now this question of the position and nature of the cerebral lesion is one to which I would wish, before leaving it, to draw special attention; and I do so because it may be—and indeed, in

my own case, has been—objected that the paralysis was simply of a “functional” character, due to temporary compression or some similar extraneous cause and not to an actual lesion—destructive lesion at any rate—of cerebral centres. I do not assert that in this particular instance it was so due, nor do I suppose it to have been so in many other cases (as Dr. Williams’s, for example,) which have been at various times recorded as genuine instances of Direct Paralysis. What I do assert is simply this—that when we find during life a distinct loss of power—motor or sensory and more or less marked—on one side of the body and on one side only, and when after death we find the only possible or only discoverable cause of this morbid condition situated on the corresponding side within the brain,—then we have an appearance, a symptomatic affection, which (whether it be of a functional or permanently organic character), for convenience and for the sake of distinctiveness, it has been agreed to designate “Direct Paralysis.”

The word “direct,” therefore, is here used merely in a relative sense, and I have endeavoured to guard against being misunderstood upon this point by adopting the prefix “so-called” in the title of the present paper. Regarded from an accurate physiological stand-point, the term may probably be proved to be a misnomer. Whether it is so or not, however, must depend upon the true explanation as to the mode of production rather than the kind and situation of the symptoms in this class of cases. Herein lies the whole of the difficulty, and it is just this explanation which the scientific physician is called upon, if possible, to give.

Lastly—as to paralysis of *sensation*, a certain amount of which apparently existed, along with that of motion, in the instance under our consideration. This fact might be urged as an objection to the case being looked upon as one of “cortical paralysis,” because cortical paralysis (including under this head the results of lesions, both those confined to the substance of the grey matter itself and those immediately exterior to its surface producing pressure or irritation—*e.g.*, “intra-arachnoideal” and “subarachnoideal” hæmorrhages,)

is not regarded as being associated with paralysis of sensation. Nor as a rule is it so : we must, however, remember not only how difficult it often is to judge accurately of the amount of impairment of sensibility in a paralysed limb, especially when the general perceptive faculties are materially interfered with, but also how varied are the symptoms—motor and sensory, individual and combined,—which are produced by morbid influences in this particular region of the brain. Hence, as we know, arises one of the main elements of difficulty in the differential diagnosis between meningeal and true intra-cerebral hæmorrhages.

For example,—characteristic manifestations, which almost universally accompany meningeal apoplexy, may at times be very slightly developed, or at others be altogether absent. Thus in the case of my own patient there was no headache, which we should certainly have expected to find : no vomiting : no spasm or rigidity of the neck or of other muscles,—unilateral or on both sides : no general epileptiform convulsions. On the other hand, symptoms usually foreign to the above condition may be present to a greater or less extent. Hemiplegia, for instance,—partial or complete—is a comparatively rare occurrence ; for paralysis of motion, where it occurs at all, is commonly general and affects all the limbs alike (even when the extravasation is limited or chiefly limited to one side of the brain), by reason of the gradually increasing and uniformly diffused intra-cranial pressure.

The whole of the facts bearing upon affections of sensibility—common and special, the precise manner in which sensitive impressions are conveyed to the centres of perception, and the definite position and extent of those centres, are as yet so open to controversy and speculation that I shall not here attempt to enter further upon the subject, but shall confine myself throughout the remainder of this paper, as I have done hitherto, to the consideration of *motor* paralysis alone.

Having then briefly reviewed some of the more probable objections which might be urged against the existence of a true (so-

called) Direct Paralysis in the case above narrated; and if it be allowed that these objections have been satisfactorily met and that the case is in reality what I have endeavoured to represent it, the final and all-important question naturally arises—How is a phenomenon of so peculiar and anomalous a character to be scientifically accounted for?

Numerous explanations have been from time to time offered by those who have investigated the subject. Some of these explanations are, I think, wholly inadmissible: others may be indeed possible but are quite hypothetical: none as yet advanced seem altogether definite and incontestable. For myself,—I am not, I fear, in a position to add to the list yet another interpretation which might be deemed in all respects satisfactory, or possess the merit of originality or of absolute precision. Such an opinion as I have endeavoured to form upon this subject I shall mention presently, and shall meanwhile devote a short space to the consideration of one or two of the more prominent views which have been hitherto promulgated by others.

I have already alluded to the explanation proffered by Hughes Bennett, Copland, and others, and originally, it would appear, by Sabouraut in Paris as far back as 1769, *viz*:—that, in the cases where the paralysis and the lesion in the brain were upon the corresponding side, the former was in reality caused by the presence of undetected changes in the opposite cerebral hemisphere. In support of this opinion is adduced the fact that, in very many instances, tumours, abscesses, softenings, and other abnormal conditions of brain tissue have been found after death where no symptoms whatever of paralysis had occurred during life; and that "a still greater number are on record, in which there was well-marked paralysis but no appreciable lesion of structure to be found after a careful post-mortem examination." These arguments, founded as they are upon well recognized facts, must be allowed to have weight so far as they go: but they do not go far enough, nor can they from their inconclusive character be considered as affording us, at least in a large majority of cases, an adequate and categorical

interpretation of an obscure pathological phenomenon.

Then the theory of the *absence of decussation* in the anterior pyramids of the medulla oblongata, which has been put forward by some physiologists, notably by Longet \* and by Mr. John Hilton, † in explanation of the occurrence of Direct Paralysis, is not only negatived by comparative analogy and the universal experience of anatomists, past and present,—but is, in the instance especially under consideration, satisfactorily disproved by the perfectly normal appearances of the parts themselves. This I fortunately had the opportunity, as has been already mentioned, of thoroughly verifying by microscopical as well as naked-eye examination; and I may here specially note that up to the present time this would appear to be the only instance—recorded at least—in which such examination has ever been made in a case of Direct Paralysis in the *Human* subject; though in the lower Mammalia, where “direct” symptoms have been artificially produced, observations have been frequently made by Brown-Séquard, as he tells us, with results precisely similar to the above.‡

This hypothesis, for it is as yet only a hypothesis, relative to the abnormal disposition of nerve fibres in the medulla, has since its first promulgation received numerous adherents; which is not indeed surprising, seeing that it affords a very simple and—if it could be clearly demonstrated—a perfectly scientific and satisfactory solution of what must otherwise be regarded as an abstruse physiological problem.

Among recent writers upon Nerve Pathology who follow and rest satisfied with the above explanation of Direct Paralysis, I notice Dr. Hammond, of New York, in his practical and comprehensive *Treatise on Diseases of the Nervous System* [6th Ed. 1876,] though he

\* *Anatomie et Physiologie du Système Nerveux*, tome i. p. 383.

† Mr. Hilton's views on this subject were first expressed in a paper read by him before the Royal Society in 1837.

‡ *Lancet*, Jan. 29, 1876, p. 159.

makes the briefest possible reference to the subject and altogether avoids any discussion upon it.

Dr. Bastian, in a concise and impartial reference to this question \* reminds us that "the fibres in the outer parts of the anterior pyramids of the Medulla do not decussate—they descend in the anterior columns of the cord on the same side, and concerning their functions we know little or nothing positively." He goes on to say—"If the anatomical structure of the cord and medulla varies" (as he believes it does) "amongst vertebrate animals, is it not possible that vices in development may occasionally occur in the nervous system of man to such an extent that the accustomed decussation of the motor tracts does not take place?" To this suggestion Dr. Bastian not unreasonably adds—"It seems worth while to entertain the possibility of such an occurrence, rather than allow a feeling of general mistrust to spring up as to the regularity of the phenomena produced by brain disease in different regions. It is, moreover, extremely desirable that the brain and medulla should be most carefully examined in any subsequent case that may occur—more especially if the lesion has implicated the corpus striatum in such a manner as to produce a tract of secondary degeneration through the peduncle, medulla, and cord." [Such an examination having been now made in at least one apparent instance of Direct Paralysis, though not one where the corpus striatum was involved, may perchance serve in a measure to do away with an element of uncertainty in some cases at any rate of the peculiar class at present under our consideration.]

As to the explanation proposed by Schiff, that in these cases there is a *re*-decussation of nerve fibres afterwards taking place, it may, I believe, be at once stated that such a view is quite problematical and entirely unsupported by anatomical facts.

Again Dr. Ferrier, in his important work lately published on *The Functions of the Brain*, while making very brief allusion to the

\* *Paralysis from Brain Disease*, p. 212-3

phenomenon of Direct Paralysis, and indeed almost implying some doubt as to its very existence as a pathological fact, seems inclined to attribute its possible occurrence—if not indeed to the actual absence of interpyramidal decussation—at any rate to some form of anatomical irregularity. He says, speaking of the cases collected by Brown-Séquard, “Even if we admit every one of them, the logical deduction from such facts, taken by the side of the hundreds of thousands of cases of cross paralysis, would be, not that the doctrine of the cross action of the cerebral hemispheres is untenable, but that there may be exceptions, just as there are exceptions to the rule that the heart is situated to the left and the liver to the right.”

That Dr. Ferrier should look somewhat askance upon this kind of anomaly in cerebral disease is scarcely surprising, for he has never yet, as he tells us, met with an instance of it in man or the lower animals, as the result of a brain lesion whether naturally or artificially produced. Hence the existence of a paralysis on the same side as the cerebral injury is apparently at direct variance not only with his experience as a physician, but also with the results of his extensive researches as an experimental physiologist. I say “*apparently*” at variance, and I use the word advisedly, because I believe that the time will very shortly come, though it may not have come as yet, when the exceptional and seemingly inexplicable symptoms of Direct Paralysis may be readily and scientifically reconciled with the main conclusions which Dr. Ferrier’s investigations have led him to adopt.

Into the general substance of these conclusions, and the marvellously patient and skilful methods by which they have been arrived at, it does not fall within the scope of this paper to enter. They have already become familiar to the scientific world through their author’s various publications; originally by his well-known article in the *West Riding Asylum Reports* (1873), his subsequent papers read before the Royal Society, and most recently by his truly *magnum opus* above referred to. I would merely here say, in connection with the subject of Paralysis from Brain disease,

that Dr. Ferrier's researches go to prove, what indeed we should have been led fully to expect, *viz.*, that the action of the cerebral hemispheres—such parts of them at least as he believes to be the centres or to contain the centres for the voluntary motor power of definite groups of muscles—is (excepting as regards certain movements of the mouth, tongue, and neck, which are bi-laterally co-ordinated from each hemisphere,) invariably a *crossed* one.

As to the soundness and positive accuracy of some of the inductions drawn by Ferrier and those other distinguished and scientific workers whose names are associated with his as exponents of the localization of the higher functions in definite ("psycho-motor,") centres in the brain, there are—and doubtless for a time at any rate will be—numerous dissentient opinions, even among men of the highest physiological acquirements.

In illustration of this I may refer to an able review of Dr. Ferrier's recent book on the Brain by an authority eminent alike in the domain of Physiology and of Philosophy,—Mr. George Henry Lewes.\* The article in question is written in Mr. Lewes's clear and logical style, and, while dealing with his subject impartially and in terms in the highest degree complimentary to Dr. Ferrier, he yet criticises with an unsparing hand what he deems to be the weak points in that author's arguments, and shows that further practical verification is required before certain of his favourite hypotheses can be universally and unconditionally accepted. "I have already suggested," says Mr. Lewes, "that the discoveries of Hitzig and Ferrier are of great importance; but only as finger-posts for anatomists seeking the pathways of stimulation, not as inductive stations for deductive inferences. All we can say at present is that electrical stimulation of certain spots is followed by certain movements; but *how* the stimulation reaches the motor nerves is as dark as before." The whole of this review by Mr. Lewes is well worthy of perusal, not only on account of its intrinsic merits as a masterly critique of a masterly work, but because it may tend to prevent many who do not possess the same physiological ex-

\* Vide *Nature*, Nov. 23rd and 30th, 1876.

perience or discriminative ability as its writer from being carried away by what he not inaptly terms the "seductive precision of Dr. Ferrier's statements and the unhesitating confidence with which only one side of a question is presented." It may also perchance assist in some measure to encourage a closer and more independent examination into the tangled web of facts and counter-facts, of antagonistic theories and speculative inferences, which at present surrounds this most difficult and complicated branch of physiological enquiry.

In the meantime, while ascribing to Dr. Ferrier his due meed of praise and acknowledging our warmest gratitude to him for the brilliant light he has already thrown upon the mysterious functions of the cerebrum, we shall do well to bear in mind his own prefatory words of caution—"We are still only on the threshold of the inquiry, and it may be questioned whether the time has even yet arrived for an attempt to explain the mechanism of the brain and its functions."

But, quitting this digression, I would ask leave to revert for a brief moment to the subject of the supposed exceptional distribution of nerve fibres in the anterior pyramids of the medulla, and to state that this supposition has been very fully discussed and (as he himself believes) finally disproved and set aside by Dr. Brown-Séquard, whose interpretation of the symptoms of Direct Paralysis is indeed altogether independent of—and at once dispenses with—such a mode of accounting for this special pathological phenomenon.

With regard to Brown-Séquard's views upon the mechanism of the central nervous system in general and of the various forms of paralysis in particular, it may be said of them, as of Ferrier's, that they are now so well known to the medical profession and have been, especially of late, so widely ventilated in this country, on the continent, and in America, that it would be superfluous, were it even possible within the limits of this paper, to make any attempt at their recapitulation *in extenso*. While, however, it is out of our power in this place to follow the details or discuss the merits of Brown-Séquard's arguments, it may be of service to recall

to mind very briefly some of the principal conclusions at which he has arrived, in so far at least as they have relation to the subject of Direct Paralysis.

Brown-Séguar commences with the somewhat sweeping assertion that "almost all our admitted notions as regard the physiology and physiological pathology of the brain are grounded upon wrong data, and, if we wish to arrive at any just conclusion, we must give up these old notions." As a foundation upon which to base his arguments he lays down at the outset a code of "propositions," which he asserts, and which I think we must all to a great extent allow, "simply constitute a summary of facts." Some of the chief of these propositions may be thus enumerated :\*—

*a.* Symptoms of brain disease (*e.g.*, paralysis, anæsthesia, amaurosis, &c.,) may exist to a more or less extent without the least visible organic alteration of the brain. *b.* A lesion of a very small part of the brain, whatever be its seat, may produce very extensive and violent symptoms: whereas, on the other hand,—very extensive lesions may exist in the brain without producing any symptoms whatever. *c.* The same symptoms may originate from the most varied lesions: whereas the same lesion may give rise to the most varied symptoms. *d.* Permanent lesions of the brain may produce only occasional morbid symptoms or symptoms varying in character and extent from day to day: again, symptoms may remain constant or may cease suddenly and entirely, the brain lesion remaining persistent. *e.* Symptoms may appear suddenly from a slowly developing lesion: or, they may appear slowly from a suddenly produced lesion. *f.* A lesion of the same kind and extent on the two sides of the middle line of the brain may produce symptoms only or chiefly in one half of the body: and,—*a lesion confined to one half of the brain may produce symptoms either on the opposite or the corresponding side or on both sides of the body.*

\*For the full enunciation and accompanying demonstration of these "propositions," see Brown Séguar's *Archives of Scientific and Practical Medicine*, March, 1873. Also *Lancet*, July 15, 1876, p. 77, *et seq.*

The gist of these propositions would seem in fact to be that (to use Brown-Séquard's own words,) "there is no necessary relation between the seat, the extent, the kind of a lesion, and the symptoms that may appear from its influence." While, however, we may not all feel justified in drawing from them this conclusion, they will at any rate serve to remind us how varied and often contradictory in character are the effects of brain disease.

Starting then on these premises, it will be at once seen that Dr. Brown-Séquard is distinctly opposed to the doctrine of the localization of the higher functions in the cerebrum—or, I should rather say, in certain definite areas of the cerebrum—according to the principles propounded by Hitzig, Ferrier, Meynert, and others of the same School. Brown-Séquard believes indeed that there are "functional centres,"—*i.e.*, "nerve cells which serve one and the same function and are endowed with the same vital properties distinct from the properties possessed by other cells;" but that these cells, instead of being located in concentrated groups or clusters confined to certain limited and sharply defined regions of the brain, are, on the contrary, very widely diffused throughout the general substance of that organ. This theory, and this alone, he argues, can satisfactorily explain very numerous cases of disease, not unfrequently met with, where extensive lesions exist in one or both hemispheres without producing any symptoms whatever, (such as paralysis of motion or sensation,) or where, in other words, certain well-defined areas of the brain in which special functions are said to be localized can be more or less injured, or even altogether destroyed, without any perceptible loss of these functions. The converse of this also takes place; so that every part of the brain is supposed to be able to give rise to a paralysis, sometimes to one form of it and sometimes to another.

It follows, then, upon the above views that one half of the brain is not necessarily the seat of the motor impulses of the will for the muscles of the opposite half of the body; but that either half, including the base, is quite sufficient for the adequate performance of all the cerebral functions: that either half, in fact, of the brain is, so to speak, a *whole* brain and may be destroyed

without involving the destruction of any of the special functions of that organ.

With regard to the mechanism by which the so-called "functional centres" are brought into communication with the motor nerves so as to produce muscular activity,—Brown-Séquard believes that very few nerve fibres are sufficient for the transmission of almost all the orders of the will to the muscles. He is led to this conclusion from the observation and analysis of a vast number of cases which he has collected, "amounting to not far from a thousand," in which little or, very often, no paralysis of the limbs existed though there was extensive disease and destruction of parts in the anterior pyramids of the medulla oblongata, the internal capsule, the crura cerebri, and other regions long recognized as the great channels of communication between the organ of will and the muscles of the body.

"What then" Brown-Séquard proceeds to ask, "is the meaning of destruction in paralysis if it is not in cases where there is a destruction of tissue in parts which are considered essential for the action of the will on the muscles?" His reply to this query is that—Paralysis in all its forms, arising from injury or disease in any part of the brain, is a purely *inhibitory* phenomenon, due to irritation in the morbidly affected spots or their neighbourhood; and that its mode of production is in all respects identical with that of the reflected symptoms from irritation of peripheric nerves in the bowels, lungs, skin, or in other parts of the trunk and limbs. The power, however, of an irritation in the brain or its appendages is acknowledged to be much greater than when it exists in other organs of the body possessing less excitability: a prick, for example of the medulla oblongata will cause immediate and complete arrest of the cardiac and respiratory—in fact of all the vital—functions.

In the same way, therefore that irritation of a peripheric nerve will sometimes produce no inhibitory or "indirect" symptoms (*viz.*, those due to an influence exerted on distant parts), or will in other instances produce any one of the many forms of such symptoms, *e.g.*, motor paralysis of the limbs, anæsthesia, aphasia,

amaurosis, and so on: so also a lesion of the brain, whether extensive or limited in degree, may or may not under different circumstances originate symptoms of this kind to a greater or less extent. Hence it becomes less difficult, if the above supposition be a true one, to afford an explanation of the very varied and often anomalous appearances, such as that of Direct Paralysis, which are found among the manifestations of morbid action in the brain.

With regard to the special class of cases of which an instance observed by Dr. Cavafy has been quoted above, it will be remembered that Dr. Brown-Séquard has pointed out that lesions of certain limited regions on either side of the cerebrum, about its base, are particularly apt to give rise to a paralysis on the *corresponding* side of the body, and that there is one spot above all others which, if pressed upon by a tumour or otherwise affected to a certain extent by injury or disease, is supposed to possess this property. This spot appears to be, as before stated, the inferior (anterior) surface of either crus cerebelli, situated immediately over a portion of the petrous bone about the point of insertion of the fifth cranial nerve into the pons Varolii. Brown-Séquard believes the paralytic affection, thus specially localized, to be most probably due to primary stimulus or irritation of the inferior surface of the middle cerebellar peduncle, exerting an inhibitory influence over a more remote centre and thus causing a "cessation of action" and paralysis. Such an explanation, however, seems at least unnecessary; and a simpler and more feasible one of the peculiar liability of the middle cerebellar peduncle to be affected as above is that afforded by a consideration of its anatomical and functional relations to neighbouring structures. For, as Dr. Bastian clearly puts it, "it is generally admitted that the middle cerebellar peduncles decussate in the pons Varolii. So that the fibres of the right side (for instance) penetrate into the left half of the pons and there come into relation with motor fibres. Such motor fibres would be those influencing the right half of the body—above their point of decussation."

In connection with this branch of our subject an interesting

paper by Dr. John W. Ogle will be found in the *Medico-Chirurgical Transactions*, (Vol. xlii. p. 403), relating very fully the particulars of a case which came under that physician's personal notice, and which is strikingly illustrative of the foregoing remarks. Dr. Ogle makes extensive reference to Brown-Séquard's views, as at that time expressed (1859), in regard to the above-mentioned feature in the history of paralysis; and he is inclined to accept them as satisfactory, though, at the same time he has evidently no thought of abandoning the doctrine of the *crossed* action of the cerebral hemispheres. He suggests as a substitute for the terms "reflected," "sympathetic," or "mediate," (which had been variously applied to a paralysis of the kind we are discussing under the title of "direct,") that of "*induced*"—as being more distinctive and more aptly designating the special relations of cause and effect in this peculiar class of cases. Dr. Ogle, moreover, mentions at the close of his paper,—“I might have added other cases of which I have notes, in whose history paralysis of motion, with or without anæsthesia, was noticed as consequent upon disease or injury of the ‘corresponding’ side of the brain.” It is to be hoped, since attention has been of late re-awakened in reference to the subject of Direct Paralysis, that Dr. Ogle will before long find leisure to publish the details of some of his cases of this nature. They are sure to prove of considerable interest and value when recorded by so accurate and experienced an observer.

I have, then, above endeavoured to give a brief sketch of Dr. Brown-Séquard's views in reference to the production of Paralysis in general. His conclusions upon this point may be finally and very concisely summed up, in his own words, under two main heads, *viz.* :—

1st.—That “*it is not possible to look upon paralysis in cases of brain disease as being the effect of loss of function of the parts diseased (i.e., of conductors or centres employed by the will in the production of movements):*” and,

2nd.—That in such cases “*there is an IRRITATION starting from the place we find diseased after death, and acting upon more or less*

*distant parts of the nervous centres, in such a way as to arrest their activity and thereby cause the paralysis."*

The intrinsic value of these conclusions we cannot, as I have said, here stop to discuss ; but concerning the former of them, at any rate, it may be safely stated that it will be received with the greatest reservation by many, while its truth will be altogether denied by others—among those who have given their careful attention to this subject. Nor could it well be otherwise. A doctrine so utterly unorthodox and subversive of long-standing and commonly recognized principles, as well as apparently so directly opposed to recent physiological investigations and carefully observed clinical and post-mortem facts, will indeed require much stronger confirmation and a more intimate acquaintance with the teachings of cerebral pathology before the validity of its reasonings can be universally established. In the present state of our knowledge, many of these reasonings will hardly, I think, carry conviction with them to the minds of most thoughtful men in face of the vast weight of accumulated evidence arrayed upon the other side. The champion of the new faith fights, it is true, boldly and with skill, and in defence of his tenets employs many able and ingenious arguments, based partly upon certain generally acknowledged data (which it would be difficult, if not impossible, to controvert), and partly upon numerous pathological and experimental observations made by himself and others. There is however, if I may venture to say so with due deference to the opinions of so eminent a physiologist, throughout these arguments of his a straining, so to speak, after evidence in their support, and a somewhat laboured introduction of exceptional cases in corroboration of his views, which savours rather strongly of the nature of special pleading. Among such exceptional cases are those of Direct Paralysis, with which we are now more especially concerned : and it may be said of them—that, although Brown-Séquard has given us an explanation which he believes he has proved to be correct, yet he has certainly not given us, as it seems to me, a sufficient reason (and indeed has made but little attempt to do so) why these exceptions should be comparatively so rare, and why a hemiplegia,

resulting from some affection of the brain, should be found almost universally occurring on the side of the body *opposite* to the lesion. There is also, besides the disregard he not unfrequently shows of the significance of counter-evidence, a certain vagueness of purpose in Brown-Séquard's reasoning and a want of close definition in his conclusions, which contrasts very markedly, and perhaps not altogether favourably, with those arrived at by his opponents in doctrine, the upholders of functional localization.

Without doubt, however, much is to be said on both sides of this difficult question; and if, on the one hand, the propositions of Brown-Séquard are too vague, so on the other hand, as it seems to me, those of Hitzig and Ferrier are at times almost too *precise*. It may appear paradoxical to speak thus of researches where precision is an element desirable beyond all things: but, at the same time, I think experience and analogy teach us that Nature herself draws her lines of functional demarcation, both in health and disease, less hard and fast in other departments of the animal economy than the advocates of localization have sought to do in the brain.

Finally as regards Brown-Séquard's views—in so far at least as they are antagonistic to the principle of Functional Localization in the Cerebrum,—I cannot but think that their acceptance would involve a retrograde rather than an advancing step in the history of Cerebral Physiology. His theory that "the character of the symptoms in brain disease is in no way dependent upon the seat of the lesion" would, if proved to be a correct one, do away in a great measure with the invaluable recognition of the relations between cause and effect. It would deprive us, therefore, of the increasing power of accurate *diagnosis* and *prognosis* (if not, in some cases, also of *treatment*), which as practical physicians we have long hoped for, and towards the ultimate attainment of which we have, especially of late, flattered ourselves that we were making steady if not rapid progress. There is, moreover, a dangerous facility offered by this doctrine for the interpretation of abnormal and apparently unintelligible phenomena in disease,—a facility which would tend rather to stifle and suppress than to foster and encourage habits of closer observation and research.

But whatever may be the errors (if they be errors) in Brown-Séguard's recent teachings, certainly want of candour or of conscientious purpose can in nowise be alleged against him. He is at the present time bent upon undoing, as he himself confesses, the very views which he held—and indeed so largely assisted in establishing—many years ago: and, while some of us may not indeed be able to endorse certain of his later suppositions, or unhesitatingly accept the principles he is now trying to substantiate; yet we cannot but admire his vigor and enthusiasm in the cause of physiological research, and be grateful to him—not only for all the valuable work he has done in the past, but also for opening out of late fresh ground for discussion and for adding an impulse to renewed investigation in one of the most abstruse and important departments of Medical Science.

In speaking as above of the opinions entertained by Dr. Brown-Séguard in reference to the production of Paralysis, I would be understood to have had in view so far the former only of his two main conclusions already quoted,—*viz.*, that this phenomenon is not an effect of loss of function,—with the accompanying renunciation of the generally accepted belief that one hemisphere of the brain is the volitional centre for the muscular movements upon the *opposite* side of the body.

With regard, however, to the second conclusion—that Paralysis is due to an *irritation* exerting an inhibitory influence over more or less distant parts of the nervous centres and thus arresting their activity,—there are probably those who will agree with me that, although in the vast majority of cases this affection is not so due, it may be—and indeed is—so in certain exceptional instances, the peculiarities of which it would be difficult, if not impossible, to explain upon other grounds. Among such instances may probably be reckoned those to which the term “Direct” has been applied, though not every one bearing this designation is of necessity to be so accounted for. Some of them may owe their symptoms to purely anatomical causes (either normal or irregular,) which, obscure and undetermined as yet, may admit of clear and accurate

demonstration when the minute histology of the brain shall have been more thoroughly investigated and understood. Already, for instance, we have seen that, in a certain class of cases where loss of power occurs on the same side of the body as a lesion affecting one of the middle cerebellar peduncles, the structural arrangement of the nerve fibres in the part affords us an adequate interpretation of this special kind of phenomenon.

While, therefore, as I have just now said, we need not go so far as Brown-Séquard does in asserting that—"Every paralysis due to an organic disease in the brain is caused by inhibition" (the effects of an irritation), and never to loss of function in the parts diseased; and that the symptoms of this affection are in every instance "secondary effects" and identical with those produced by an irritation of centripetal nerve-fibres in the bowels (*e.g.*, by worms), the skin, mucous membranes, &c., (the so-called "reflex paralysis");—we must, nevertheless, I think, feel compelled to allow that in certain cases in which paralysis appears, and these not so few perhaps as might be generally supposed, this explanation is the most feasible and satisfactory—or indeed the *only* satisfactory one—that, in our existing state of knowledge, we have to offer. Such a view, moreover, seems not only consistent with numerous daily observed facts, but is also, as far as I can see, by no means necessarily irreconcilable, as Brown-Séquard seems to argue, either with the doctrine of the crossed action of the hemispheres, or with that of the endowment of certain special regions (centres) of the brain with special and clearly defined functions.

If, then, we acknowledge the power of irritating and inhibitory influences to give rise, on occasion, to symptoms of paralysis—whether "direct" or crossed,—we are in the next place, led to enquire what is the precise mechanism and what the mode of action by which phenomena, so varied in intensity and in kind, are actually produced? and this is a question which it is by no means so easy to answer. Brown-Séquard, in reference to this matter, remarks—"As we know that irritations vary considerably in their power in different individuals, and in the same

individuals at different times, or in different parts of the body, or, besides, in corresponding parts of the two sides, we can easily understand that one side of the brain in the same individual will react powerfully under irritation while the corresponding or rather the homologous parts on the other side will not react. Such a thing is not very frequent; but after all, if we consider that the nervous centres are double organs, a difference in the intensity of the excitability of one side, compared with that of the other side, is not more wonderful than the difference of the excitability of parts of the nervous system of one man compared with that of similar parts in another man."

With a view to test in some measure the character and effects of certain irritative and destructive agencies applied to the surface and substance of the brain, and to verify, if possible, the results obtained by Dr. Brown-Séquard in his experiments upon the lower animals, I determined to carry out for myself a series of such experiments. I was induced to attempt this inasmuch as Brown-Séquard had stated that he was able, almost at will, to produce the appearance of Direct Hemiplegia in animals, and that "a paralysis on the side injured always follows certain injuries of the surface of the brain." The particular form of lesion, he says, which has most power to produce the above effect consists in a *burn* of a part of the surface of one hemisphere of the brain, the middle lobe affording the most striking results—the anterior the least so. Other lesions, however, and also of more deeply seated portions of the cerebral substance—such as the corpus striatum, the walls of the lateral ventricles, &c.,—have also, he affirms, this power. The paralysis produced as above mentioned is said to be slight, and variable as regards its seat, usually existing in one of the limbs on the side of the lesion, sometimes in both: it also appears at times in the abdomen, thorax, face, neck, or bladder. If, however, the burn of the surface of the brain be deep and very extensive, paralysis is liable to be produced in all four limbs to a greater or less extent, but greater in those on the side opposite than on that corresponding to the brain lesion. In the *Archives de Physiologie Norm. et Pathol.* (Nov. 1875).—Brown-Séquard, after the

performance of a large number of experiments, has described as a constant effect of an irritation of the surface of the brain on one side, by a heated iron, a paralysis of the cervical sympathetic nerve on the same side, accompanied by the various phenomena which follow section of that nerve. The degree of paralysis he found to be in proportion to the amount of the stimulus, and to the extent of surface to which it had been applied. Similar results also were similarly obtained in the case of the scalp and meninges. From these observations he believes it is demonstrated that "the lobes of the brain are capable of excitation by mechanical and thermic—just as they are known to be by electric—stimuli."

Of the numerous experiments made by me (chiefly upon rabbits and guinea-pigs), some were total failures and vitiated by such accidents as death under the anæsthetic, wounds of the cortical or subjacent cerebral substance, or severe and exhausting venous hæmorrhage causing great depression of excitability in the nervous centres: others were almost negative in their results and afforded no specific appearances of value: while scarcely any—if any—produced the distinctive phenomena indicated by Brown-Séquard and which it was my especial endeavour to obtain. The mode of opening the cranium was that usually followed in these experiments,—*viz.*, by trephining in one or two places with a small instrument, and then gradually extending the exposed surface by means of fine bone-forceps or strong scissors. I may add that an anæsthetic (chiefly ether) was used whenever possible, and all needless infliction of pain avoided.

It is not, however, my intention in the present paper to enter into any details of my experiments; partly because I have already trespassed too largely upon the space at my disposal, and partly because the series has not yet been completed and scarcely sufficient data obtained upon which to found definite and reliable conclusions. Moreover, although these observations were made before the "Vivisection Act" came into operation, it is perhaps undesirable, in the present state of popular feeling upon the subject, to publish the results of experiments upon animals unless they can

be safely regarded as of positive scientific value. I shall hope for opportunities to carry out further, and upon stronger animals, the researches I have commenced in connection with the production of paralysis, when the present vexatious opposition to such researches (for it can hardly be otherwise characterized) shall have been in some measure withdrawn: and it is earnestly to be wished, for the sake of physiological and medical science generally in this country, that the authorities, in whose power it so largely lies to encourage or retard its advancement, will shortly see their way to afford greater facilities for the development of such invaluable aids as carefully conducted vivisections alone can give towards the attainment of a greater knowledge of the animal economy.

In connection, then, with my own experiments upon animals I shall say but few words in this place. They can hardly be deemed satisfactory, in so far at least as they tend to confirm the results obtained by Brown-Séquard, or to demonstrate the special phenomenon of Direct Paralysis on account of which they were undertaken. On the contrary; in almost every instance where a definite loss of power (in the limbs at least) was produced, it appeared upon the side *opposite* to that cerebral hemisphere in which the exciting lesion was located. In two experiments only, out of the whole of the series, did I succeed in obtaining even the slightest evidence of *Direct* Paralysis: and, though certain of the symptoms in both the cases in question were apparently of this character, they were scarcely sufficiently well marked, and were at the same time so extremely transient, that—having regard also to the difficulty of estimating with exactness, in a small and non-convoluted brain, the depth, extent, and relative position of the lesion,—I should hesitate to draw from such limited data, and without further verification, inferences which could be put forward as reliable or of value. The transient character of the paralysis was very noticeable in all the experiments where this affection was artificially produced, whether by the actual cautery or other agent, and was especially so when rabbits were employed, as they were in the two cases especially alluded to. This is a fact observed and commented upon by

Professor Ferrier, who also draws particular attention to the relative differences, in the effects of destruction or irritation, between the cortical motor centres and the corpora striata in different kinds of animals. He shews that in man and the monkey, for example, there is little, if any, difference perceptible between the complete destruction of the cortical motor centres and destruction of the corpus striatum: while in the case of dogs this difference is very distinctly marked. "In rabbits, again, the destruction of the cortical motor centres produces a less marked and more transient affection of motility than even in dogs." "These differences," says Ferrier, "can only be satisfactorily accounted for on the principle that animals differ greatly in respect to the degree in which the motor activities are independently organised in the mesencephalic and lower centres." Hence it becomes evident how necessary it is "to guard against the extension of conclusions arrived at from experimentation on one class or order of animals, to others and to man, without due qualification and restriction."\*

*Apropos* of Dr. Ferrier's observations upon the lower animals,— I may here remark that in the course of my own limited researches (though these were conducted solely for the purpose of determining, if possible, the effects of various irritative and destructive lesions in the production of Direct Paralysis, and not in any way of illustrating the localization of the cerebral functions,) several appearances did incidentally occur which seemed to point very distinctly to certain definite regions of the cortex of the brain as the starting points for certain muscular movements; and, in not a few instances, results arrived at by Ferrier in his recent investigations, as well as by other observers working in the same direction, were very clearly corroborated. Among other propositions experimentally demonstrated by this physician and distinctly verified by myself are such as the following:—that the corpora striata are respectively the centres for the muscular movements of the opposite side of the body, and that destructive

\* *The Functions of the Brain*, p. 249, *et. seq.*

lesions of these ganglia give rise to crossed hemiplegia; while powerful excitation of either one of them causes rigid rotation of the head to the opposite side with, in most cases, general pleurosthotonos (unilateral tonic contraction of the muscles of the face, neck, trunk, and limbs), the flexors prevailing over the extensors; irritation of the right corpus striatum producing left pleurosthotonos, and *vice versâ*. [These effects are somewhat less strongly marked as has been already mentioned, in rabbits than in many other animals.] From the above facts regarding the corpus striatum, coupled with that of sensation remaining unimpaired, it would appear, as Dr. Ferrier says, that this ganglion "has, beyond all doubt, a purely motor signification;" and he further infers that in it "there is an integration of the various centres which are differentiated in the cortex." Another of this observer's conclusions I was able to verify—that certain lesions of the cerebellum give rise to manifest disturbances of equilibrium, as also to various rolling movements of the eyeballs and distortion of the optic axes. In one instance, however, I noticed violent and persistent *nystagmus*, defined by Ferrier as a "choreic or epileptiform affection of the cerebellar oculo-motorial centres," where, as far as I could ascertain, there had been no irritation applied immediately to—or in the neighbourhood of—these centres. I had also opportunities of satisfying myself of the fact, first enunciated by Dr. Broadbent, *viz.*, that, though the action of the respective halves of the brain is in general crossed, certain movements of the mouth and neck (I cannot speak for the *tongue*) are "bi-laterally co-ordinated from each cerebral hemisphere."

Numerous other results of a physiologically interesting and instructive character came under observation during my experiments, but I must not now further digress by referring to them. These results, though they have no immediate bearing upon the present subject of discussion, have nevertheless indirectly served to strengthen my own previous conviction that, however we may attempt to explain the anomaly of Direct Paralysis, we have at least no reason to abandon the doctrine of the crossed action of the hemispheres, or the belief that this crossed action is directly

due to the decussation of nerve fibres (voluntary motor conductors) in the anterior pyramids of the medulla oblongata.

In this most important respect, then, I venture to differ entirely from Dr. Brown-Séquard, while otherwise accepting in great part his proposition that the inhibitory influence of an irritation, acting as already described, affords us a reasonable explanation of sundry anomalous instances of brain disease, including those of so-called 'Direct Paralysis.' Take, for example, such a case as that observed by Dr. W. Williams, a brief summary of which has been given earlier on in this paper; a case—which, from its eminently typical and uncomplicated character, is especially deserving of comment and attention. Here there was clearly no destruction of parts: no absolute loss—though a temporary arrest—of function. The whole extent of the original fracture was only half-an-inch in diameter, and there was no depression of bone, enough to cause pressure on the subjacent brain substance, except at one point where a minute spiculum (one eighth of an inch in length), projecting from the inner table, impinged upon the surface of the dura mater, though without piercing or lacerating it, and "not being even sufficient, as a subsequent examination proved, to produce any apparent change in the membrane itself." As soon as this spiculum was removed by the trephine, the paralysis began at once to disappear, so that at the end of a fortnight the patient expressed himself as feeling "all right:" the fits of epilepsy ceased also, four or five only having occurred since—and very shortly after—the operation. The convulsions were, like the loss of power, unilateral and *on the same (left) side as the cerebral injury*, and were invariably preceded by a distinct 'aura,' a sensation of numbness, commencing in the middle finger of the left hand and travelling rapidly up the arm to the head. These epileptic seizures, accompanying—or more probably initiating—the hemiplegia ("The Epileptiform Onset"), form in this and similar cases an interesting and important feature. They shew the power of an apparently very slight irritative agent to produce not only phenomena of *arrest*—but also of abnormal *increase*—of activity; the latter being, as Dr. Hughlings Jackson has so ably tried to show, distinct

expressions of exaggerated function, dependent upon the explosive action of grey matter, or what he has designated "*discharging*"—in contradistinction to "*destroying*—lesions" of the cortical centres.

Now if it be allowed, as it surely must, that the symptoms—epileptic and paralytic—in this remarkable case were due to an *irritation* set up by the spiculum of bone; and, if it be *not* also allowed that they were produced in the special manner indicated by Brown-Séquard,—how otherwise can they be accounted for? For my own part, I confess, I am unable to find any other answer to this question. It seems impossible, in the present case at any rate, to explain away the facts upon the principle that some other lesion, or lesions, capable of giving rise to epilepsy and paralysis, existed—but were undiscovered—in the *opposite* hemisphere. This hypothesis is disposed of at once by the simple incident that the moment the only palpable source of irritation was removed, immediate relief of the morbid symptoms took place; though it may doubtless hold good in some—perhaps in many—of the cases which have been from time to time recorded as examples of Direct Paralysis. We know well enough that not unfrequently in the post-mortem room instances are met with where a grave lesion in the brain is found on the side corresponding to the paralysis during life, but where further examination shews us that *another* lesion, or more than one, of equal—or, it may be, of greater or less—apparent gravity exists in the *opposite* hemisphere. Such a case occurred in my own practice very recently. The patient had complete paralysis of both limbs on one side, the other side remaining entirely unaffected. The autopsy shewed a large patch of softening in the corpus striatum of the *same* side as that on which the paralysis had existed, and at first sight I thought I had come across another instance of 'Direct' Hemiplegia. Further search, however, disclosed in the opposite corpus striatum a second patch of softening, distinct from the first—named and *very much smaller in extent*, though still sufficient (judging from analogous cases) to have occasioned the symptoms, as most probably—or, at least, quite possibly—it had done. The case is of interest as shewing how extensive an injury to an important motor centre can at times exist without (of necessity)

giving rise to any obvious symptoms; and also how requisite it always is to examine thoroughly and minutely the substance of *both* cerebral hemispheres in every case of doubtful import.

And this brings me to offer a few further and final remarks upon the case of Direct Paralysis specially observed by myself, which, though scarcely perhaps so simple and typical in character as Dr. Williams's, yet demands some attempt at explanation. That the symptoms of loss of power were due, in the first instance, principally—if not entirely—to the effects of intra-cranial *pressure* is, I think, from their nature more than probable. There are several features in connection with the earlier stage of the case which seem to support such a view. The patient when first found lying in the street was, as ascertained at the time, in a state of almost if not quite complete coma, though this had in a slight measure passed off when she had reached the Hospital and the stomach pump came to be applied. It has been stated that at this time the left pupil was "widely dilated;" the right one being, "(if anything) very slightly contracted,"—which latter is doubtful if we remember how difficult it is to judge accurately of the normal condition of one pupil when the other is greatly dilated or otherwise very materially altered in dimension. This excessive dilatation of one pupil with absence of reaction to light, is, as we know, an almost universal accompaniment of partial cerebral compression, *i.e.*, compression of the same side of the brain only. Had this been general and more or less uniformly diffused over the surface of the two hemispheres, we should certainly have expected to find *both* pupils dilated and of pretty similar dimensions. The state of coma gradually passed away and with it apparently in great measure the evidences of pressure; for, on the morning following admission into hospital, "the pupils were both equal and normal in size and acting fairly under the stimulus of light." There was also now (and this is worthy of note) a total absence of *pain in the head*, which is recognized as an invariably concomitant symptom of compression of brain substance, and attributed to tension of the highly sensitive dura mater. The paralysis, however, of the left side remained, and there was, in addition a corresponding ptosis with external strabismus.

From these facts I am disposed to conclude that the immediate pressure exerted by the blood-clot upon the surface of the left hemisphere, though probably the primary,—was not the only, perhaps not even the principal—factor in the production (or, at any rate, the maintenance) of the paralytic symptoms. These symptoms certainly became less marked after the first shock of the hæmorrhage had passed away, but they nevertheless remained persistent and distinct up to the date of the secondary effusion which gave rise to their complete renewal, together with coma, and, within a day or two, to death. But here, even though we were to allow that the hemiplegia was due solely and throughout to pressure, we should naturally expect that this pressure, if able to act at all upon the *right* hemisphere of the brain, would at least have expended the greater part of its force upon that one (the left) in which it actually existed, and would thus have given rise to symptoms on the *right* rather than on the left side of the body. There still, therefore, remains the necessity for explaining the anomalous appearances upon the side corresponding to the cerebral lesion.

I would, then, myself suggest as an interpretation of the present case that, after the initial effects of the compression had disappeared (the brain substance being in no way injured), an irritation, set up of the presence of the blood-clot, yet remained, which extending over and effecting a greater or less number of motor centres in the cortex, exercised therein an inhibitory influence causing arrest of activity—in fact *paralysis*—in the limbs. So far (except as regards the admission of “motor centres”) I should be mainly in accord with Brown-Séquard’s views; but I would dissent from him in saying further that—in this case, as in that recorded by Dr. Williams, and indeed in *all* cases where paralysis is found on the same side as the brain lesion,—this affection is due *not* to an influence conducted throughout its whole course in an *immediate* or *direct* manner (using the word “direct” in its literal acceptation,) *i.e.*, *vertically* downwards from the seat of such lesion to the corresponding side of the body: but that, on the contrary, this conduction takes place through the medium of the *opposite*

cerebral hemisphere ; that, in other words,—a paralysing influence, starting from the original point of an irritative or other lesion in either hemisphere, is propagated to centres or conductors in the opposite—and not in the same hemisphere ; so that a *crossed* action is always thus maintained.\*

The question of the process and means by which this propagation of morbid impressions from one hemisphere to the other is effected (*if it is so effected*, as I myself believe,) involves a problem very difficult to solve ; probably it is one which, with our present comparatively imperfect knowledge of cerebral physiology, does not yet admit of solution. The true explanation, when it shall have been satisfactorily demonstrated, may probably be found to rest upon purely structural grounds and upon some abnormal or adventitious deviation or connection of motor nerve fibres within the substance of the hemispheres themselves, apart from any absence of decussation in the medulla. We may, at any rate, reasonably entertain the supposition, so far as it goes, that the actual transmission of morbid influences in these cases takes place through some of the systems of commissural nerve-fibres (as of the corpus callosum) connecting the respective halves of the brain together, or of the so-called "associating fibres" which form links of communication between the associated actions of different regions of the cortex. On the other hand, however, it may possibly yet be proved that the term "direct" is after all a correct one and strictly applicable to symptoms appearing on the side of the brain lesion. To be so it must be definitely shewn (which so far it never has been, though frequently suggested as probable,) that those fibres in the outer part of the anterior pyramids of the medulla or others which do not decussate, are capable on occasion of forming—and, in the anomalous class of cases above

\*If this view be correct, it follows (as I previously suggested it might do) that the term "direct," applied to a paralysis on the side of the brain lesion which gave rise to it, is in reality a misnomer ; and that this form of hemiplegia would, in strict physiological language, be more fittingly described as "*indirect*" "*mediate*," or, as Dr. Ogle has proposed, "*induced*." I may therefore, to appear consistent, again state that throughout the present paper I have used the epithet "direct" in a purely arbitrary and conventional sense, and as expressive of the *apparent*—rather than the actual—relation existing between cause and effect.

alluded to, do form—the actual channel of communication between the centres of volition in the cerebrum and the muscular movements upon the corresponding side of the body.

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In conclusion, I would say that—whether on the one hand, we incline to accept, wholly or in part, the doctrines advanced by Brown-Séquard, or, on the other hand, that of the Localization of the Cerebral Functions,—and whatever should ultimately prove to be the true explanation of a paralysis appearing on the same side as the seat of its causation in brain,—it is assuredly a phenomenon of a sufficiently remarkable and important character to render it well worthy of further clinical observation and experimental and pathological research. It may indeed be that, as Dr. Bastian remarks, “We, as practitioners of medicine are in a measure independent of those difficult questions which at present perplex the cerebral physiologist—questions as to whether the symptoms in cerebral lesions depend upon the mere abolition or stimulation of function of the part involved, or whether they are due to a stimulating or inhibitory effect upon functions of distant parts, brought about either by obscure reflex agencies, by unknown anatomical communications, or by pressure indirectly occasioned.” Nevertheless, practitioners though we be, it surely behoves us, as members also of a scientific profession, to be able to give a reason for the faith that in us. While, therefore, we must ever in the first instance have regard to physiological truths, it is doubtless (especially in these days of opposition to experimental enquiry) to the bedside and the post-mortem room, rather than to the laboratory, that we must look for the establishment and confirmation of those fundamental principles which are to guide us in the accurate diagnosis—and frequently, it is to hoped, to the effectual treatment—of cerebral disease. Fortunately for us all—there is no lack in the present day of earnest and skilful men who are labouring steadily and constantly towards the attainment of this goal; and with explorers in this field of research such as Hughlings Jackson, Charcot, Ferrier, and a host of others both in

Great Britain and abroad, we may not have long to wait for the solution of the great problem of the Functions of the Brain. Already, much has been accomplished and a wide pathway for further enquiry opened out: it will, therefore, be the pleasure and the privilege—as assuredly it is the duty—of every one who has the interests of Scientific Medicine at heart, and in whose hands the opportunities lie, by careful clinical and pathological observation to render all possible aid in the further development of a work so auspiciously begun—and, as far as it has yet gone, so ably and successfully carried out.

# A PLEA FOR THE MORE FREE REMOVAL OF CANCEROUS GROWTHS.

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*(The substance of a paper read before the Lancashire and Cheshire Branch  
of the Brit. Med. Assocn. 1877.)*

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MR. PRESIDENT AND GENTLEMEN,

There are certain subjects, such as lithotomy, herni a stricture and cancer, which are of never-failing interest to the surgical mind. Some of these derive their importance from the fact that each individual case has its own peculiarities. We never go to a hernia for instance without feeling that it may present perfectly unexpected features. Stricture and stone again engage our attention by reason of the numerous and ever varying methods of treatment applicable to these diseases. Cancer, alas! claims our consideration from the painful fact that we have hitherto been able to do so little for it. In the days before anaesthetics manual dexterity and rapidity in operating were absolutely essential to make a man a first-rate surgeon. These were the days of the brilliant operators; of the men whose swift and unswerving work with the knife was the constant theme of conversation with their students for the rest of their lives. But, looking calmly at the matter from our present point of view, I am sure that the prominent idea then after an operation was not so much "what has been done?" but

"How has it been done?"—When, after many painful hesitations, the unfortunate patient had laid himself on the operating table, had clenched his teeth and screwed his courage up to the last point, without doubt the surgeon's engrossing thought must have been "How shall I most rapidly remove this diseased part and set this miserable wretch free from his agony?"—Hence it happened that operations for cancer were almost always done imperfectly: necessarily imperfectly, because hurriedly. The timid patient shrank from the ordeal of total ablation of the breast. The surgeon too gladly contented himself with removing the scirrhus growth from it, and endeavoured to persuade himself that he had *nearly* got it all out and possibly it might not return soon;—they must hope for the best, and after all he had only left *such a little bit behind*. And so the wound was stitched up and the patient soon got well;—only as soon to discover another hard lump in the cicatrix or a little swelling in the armpit, and then the surgeon shook his head and there was nothing more to be done. Hence the reason why operations for cancer were decidedly not in repute with the public, while many very good surgeons openly stated that the prolongation of life by operation was on the average so short that it was a question if the game was worth the candle. I feel sure there are few practitioners who received their education during the first half of this century who do not look with a very despairing eye upon the removal of a breast. Only seven or eight years ago, while looking through the Medical Times and Gazettee, I saw a letter from apparently an elderly practitioner headed "Query as to amputation of the cancerous breast." He narrated how Mr. Cline and Sir Everard Home, both men of great experience and sound judgment would scarcely ever consent to the removal of a scirrhus breast under any circumstances. He said it was an operation still recommended and performed by several eminent surgeons, although he personally found that the least experienced surgeons were the most forward to urge removal; and he concluded by asking if there was any permanent benefit to be got by cutting out a cancerous breast even in the early stages of the disease.

But anæsthetics have changed all that. In surgery we have exchanged brilliancy for patient daring. We do not look at our watches now to see how many seconds elapse from the time the operator takes the amputing knife in his hand till the limb falls to the ground. We have blocks of woods to care. It is amazing what an amount of chopping and what losses of blood the human body will endure provided no great arteries or nerve trunks or big bones be divided. It is when these are slashed through, as in thigh amputations, that the terrible shock comes. Hence in examining a cancer with a view to removal almost the only point we have to consider is whether any of these almost vital structures have been seized upon by the disease. If this be not so, and if we have a fairly healthy subject to work upon, the whole thing resolves itself into a matter of dogged resolution and anatomical knowledge on the part of the operator. He must set to work to cut and tie and cut and tie till every particle of the disease is dug out. Æther will keep the patient's heart acting during the most prolonged operation :—antiseptics (of any sort and applied it any fashion you like) will ward off most after risks. The advantages which we of today possess over our immediate predecessors in the facilities for operating are immense; the things we can do were never dreamed of by our fathers, and so it comes to pass that our views regarding the removal of cancers are far more hopeful than theirs, and there is hardly a surgeon now who would deliberately advise a patient with a removable cancer to refuse operation. Nevertheless our foreunners gave operations for cancer a bad name, which has stuck to them; and the public pertinaciously adhere to any medical idea which they have once fairly grasped. They refuse to be told that cancers can be removed and ought to be removed, and so, as if we had not enough to do to fight the disease, we have generally to begin by a preliminary assault upon the patient's scruples. How often has each one of us seen some poor creature actually nursing the fatal stony lump in her breast, persistently concealing it, hoping against hope that it may not after all be a cancer? Is it mere fear of being operated upon that causes this feeling? I hardly think so, because

in other cases where even more serious operations have to be faced the same almost abject dread is not manifested. No, the woman does not fear being operated upon:—she fears being operated upon uselessly. Almost from her childhood she has heard what a fatal thing cancer in the breast is and how operations are of no good, but only torment the patient, and from the very moment she begins to suspect the existence of the disease this thought is ever with her. At last concealment cannot longer be maintained, the entreaties of friends or severe pain or ulceration with its fetid horrors compel her to submit when too late. An imperfect removal is accomplished, followed by a speedy return of the disease, and the patient goes down to the grave wishing she had never been touched and leaving a legacy of doubt and fear to all her female friends who have heard of her melancholy case. I think it is our bounden duty, whenever circumstances give us the opportunity, to disabuse the lay mind of this unreasonable dread. By doing so the public will come to regard an operation for cancer in the same light as they do an operation for stone or for cataract; a bad job at the best, they will say, but still with a prospect of ultimate cure. They will come to us when their cancers are small and request us to remove them, and, if we could only extirpate them freely when small, what might we not expect? Have you ever imagined what the results would be if all cancers were thoroughly excised when they were no bigger than peas?

Of all the forms of cancer epithelioma seems the most local. It spreads, it is true, but always by creeping from point to point. Certain of the round celled sarcomata on the other hand (such as are commonly called medullary or encephaloid cancers) appear first perhaps in the periosteum of a limb bone, but shew a painful tendency to crop up, afar off from their original site, in the lungs the liver, or the uterus. They, more than any others, seem to be evidence of a diffused or constitutional cancerous state of body. Scirrhus seems to be midway between these two forms. There is a hard, gristly scirrhus occurring chiefly in thin elderly subjects which is slow of growth, and as local as epithelioma, while there is the juicy, soft variety occurring most frequently in the younger

and fatter female which grows rapidly, spreads its roots quickly through the tissues, infiltrates the skin and soon affects the neighbouring glands. I am firmly of opinion that if an epithelioma or a scirrhus of the hard, slow-growing form be early and freely removed that its occurrence in the original site need not be expected. I have during the last ten years removed many epitheliomata from the lip in private practice and have never once heard of a return in the lip itself, while I can point to several cases of hard scirrhus of the breast in which the whole mamma has been removed, which have now remained well for years. There is no mystery about this, all that is required is to take the precaution to cut freely through sound textures. *But this is exactly what surgeons will not do.* I have over and over again watched a surgeon remove a breast and have seen him make his incisions so as to include a little elliptical bit of skin containing the nipple after which he would proceed to scoop out the breast so as to get nice flaps which fell easily together and covered in the wound. He never considered that in the brawny, dotted skin over the tumour were already implanted the germs of the disease and that the most dangerous thing he could do was to leave plenty of skin,—skin, one of the tissues above all others, along which cancer cells love to make their way. I can safely affirm that in the great majority of operations for cancer which I have seen, and they are not a few, the cancer has not been removed root and branch. When the tumour has been carefully looked at, after the excitement of the operation has been over, there has generally been found some spot where everyone wished that a bit more had been taken away. Not long ago I was looking over some old illuminated manuscripts. Among other pictures of the tortures inflicted upon the faithful one was in which a horrid wretch was represented with a great crooked knife mangling the body of a devout female. He had slashed off one breast, the former site of which was represented by a great bleeding chasm, while with one hand he was clutching the remaining organ and with the other was preparing with one sweep to lop it off also. I could not help wishing that I had this picture to hang up in the students' room

in our school ; not *in terrorem*, but as my idea of what most amputations of the breast ought to be ;—not a niggling, half-and-half digging out of the tumour, but a radical sweeping away of the whole mamma skin and all. Had this been done to all cancerous mammæ some patients doubtless would have died under it : many scores would have been spared the agony of a recurrence of the disease. Some years ago I noticed a short paper in one of the Medical Journals. It was entitled “A new operation for cancer of the breast.”—I eagerly read it hoping to find some startling novelty described. But no ;—all I could discover was directions to remove the whole breast with the greatest thoroughness and freedom, and even to take away the pectoral fascia and part of the pectoralis major itself if necessary. At first, I felt a little indignant with the author and thought it savoured somewhat of quackery to put such a title to his paper, but the more I considered the matter, the more after all did his title justify itself. Thorough and radical removal *was* a “new operation ;” at all events it was so seldom performed as to warrant the name. Hear what the late Mr. Charles Moore, who had immense experience in cancer at the Middlesex Hospital, said in a paper on “Inadequate operations on Cancers,” written ten years ago. “Taught “without doubt by foregoing failures, our surgical ancestors “adopted method of operating which might well have been “expected to prove effectual against a local recurrence of the “disease. They transfixed the base of the mamma, and, raising it “with ligatures, swept off the whole organ, together with all the “skin that covered it. The proceeding had a barbarous appearance enough, but it was promising ; and, if their knowledge of “the disease had led the surgeons of the time to adopt it before “the skin was hopelessly infiltrated, they must have met with more “success than they appear to have done. Postponing, however, “all operation until the skin was brawny and filled with tubercles, “and the deeper textures were involved without limit, they failed “too often, with even such extensive cutting as they adopted to “comprehend the entire disease. It was a mistaken kindness “which led to a change of this mode of operating. Under the

"influence of a clergyman, who expressed what must have been  
"a prevailing horror at such Amazonian surgery, the practice  
"was changed to an incision in the integument, which was  
"reflected in flaps and brought together again after the removal  
"of the cancerous tumour. There could have been no diminution  
"of suffering by this prolongation of the operation, and what was  
"gained by it in neatness was lost in life. With the remains of  
"the breast, as well as in their own texture, the flaps enfolded  
"fragments of diseased substance and cancer soon reappeared.  
"It was an operation proper to innocent tumours, which can be  
"removed without the needless mutilation of extirpating the  
"breast, but was quite inapplicable to cases of mammary cancer."  
Again in the same paper he says "By the issue of operations thus  
"conducted opinions on the nature of cancer are always qualified  
"and sometimes formed. When they have been extensive  
"operations, there is an appearance of reason in assuming them  
"to have been complete. But while, on the one hand, severe  
"operations reduce the patient and favour the growth of cancer,  
"on the other hand, it must be considered that operations are not  
"adequate merely because they have been large. A complete  
"extirpation of cancer could hardly be looked for except from  
"a large operation for a recent and limited disease."

I venture then to put in a plea for the more complete extirpation of primary cancerous growths. In a short paper one cannot argue long nor adduce many facts or statistics:—one must dogmatize on the results of personal experience. My contention is that many forms of cancer have obtained an undeserved reputation for danger from their strong tendency to "recurrence," the truth being that for that term should be substituted "steady growth."—The remains of the tumours left behind at imperfect operations simply grow and multiply. The disease does not "recur" or come back, for the best of all reasons that it is not taken away.

Now, concerning secondary cancerous growths in glands. The introduction of anæsthetics, as I have just said, has had the effect of inducing modern surgeons to attempt the removal of primary cancers of large size and in dangerous situations with a frequency

which was never the case in former days. And although I am confident that in most instances these removals have been incomplete, yet we have all seen cases where the primary disease has never recurred at all. What a bitter disappointment it is both to surgeon and patient to find all undone by the cropping up of the disease in neighbouring glands months after the removal of the original growth! I was taught some fifteen years ago that such cases were hopeless, inasmuch as lymphatic affection meant that the disease was thoroughly ingrained in the constitution and ineradicable. I daresay in this room are some old pupils of Syme, who can remember that in hospital the detection of a lump under the jaw or in the armpit in a case of cancer was followed by a shrug of the shoulders and a quiet intimation to the house-surgeon to dismiss the patient as kindly as possible. There was nothing more to be done. Syme's personal influence over his pupils was very remarkable, and I feel sure that the hopeless view which he took of secondary, glandular cancers has long deterred surgeons, who were trained in the Edinburgh School, from attempting their removal. But if it can be practically shewn that many cancers are merely local and that mostly all of them kill by local manifestations, such as exhausting discharges, hæmorrhage, pain or eating into vital parts, why should we not pursue them again and again, and so to speak stamp them out, if so be that we can do so without killing the patient? We remove a cancerous breast: - at the end of six months there is no recurrence there, but in the axilla is a firm, moveable but infected cluster of glands, which no doubt contained cancer germs at the time of the first operation. What are we to do? Are we to sit still and let this grow till it fills the axilla, becomes adherent and immoveable and seizes on the great vessels and nerves? Most distinctly not. The whole question is a purely anatomical one. If the great vessels and nerves are still free and we can get the whole mass out, by all means let us have it out. To Lister and Bryant we owe much for the persevering and bold manner in which they have attacked these secondary glandular growths, and have shewn that all that is wanted is careful, patient dissection. The three chief sites where we are called upon to remove infected glands are the groin in

cancer of the penis, under the angle of the jaw in cancer of the lip and in the axilla in cancer of the breast. Under the angle of the jaw it is noticeable that the glandular lump shews a rather rapid tendency to soften and become firmly adherent to the jaw itself. But this need not deter us. Cut a piece out of the jaw too. I have done this twice and have seen it done three or four times in the Infirmary here. Unfortunately I cannot tell what became of the patients afterwards. The axillary glands, having a large loose space to grow in, remain much longer non-adherent. Unless there be some serious adhesion their removal is by no means so difficult or dangerous a process as it used to be considered. If the breast is being removed at the same time, a good plan is to leave its upper angle still attached and carry it and the glands all away in one piece. A good leverage upon the glandular mass is thus obtained. The incision should run upwards a little below the edge of the great pectoral muscle as far as the biceps so as to give thorough access to the cavity. A few touches of the knife reveal the glands. Then straightway before doing anything else proceed with caution to find the axillary vein. By doing this you at once lay bare the dangerous spot in the dissection and thenceforth your mind is set at ease, and you are no longer oppressed with the dread of suddenly plunging into it, since it can be quite readily pressed aside out of harm's way. When the patient's arm is drawn well up, as it is at this stage, it is surprising how near the vein is to the surface, and this must be carefully remembered. I have more than once seen narrow escapes, and once I saw it opened. The vein is very unlike what it is in the dissecting room. Here in the living body it conceals both artery and nerves, and very likely the latter are not seen at all during the whole operation. Having fairly made out its outline all further work must be done with the handle of the knife and the finger. No more cutting with the blade. With the handle you define the vein and loosen the glands and, as you do so, you will see tributary vessels running into the latter and larger vessels through them to the chest wall or the muscles. With an aneurism needle loaded with carbolized catgut let everything be tied that looks like a vessel in two places

and cut between the points of ligature. By so doing all loss of blood will be saved, and, what is of more consequence, the dissection will be as clean as if it were done on the dead subject. Not a vestige of a gland must be left, even if you go as high as the clavicle in search of them. Mr. Lister shewed that, when necessary to expose the very top of the axillary space, this could be done by dividing the pectoral muscles without any harm resulting. I have but seldom seen occasion to do this, but the other day I assisted Mr. Bickersteth at a case where it seemed advisable. The breast had been removed and the axilla cleared out, but, when apparently done, a small hard gland was found just below the clavicle adhering with painful firmness to the axillary vein. It was rather too risky a proceeding to remove this without seeing it plainly and so both pectorals were divided, the very top of the axilla freely exposed and the gland safely picked off. You may imagine what a tremendous wound all this proceeding involved. It was, however, done thoroughly antiseptically, and a fortnight afterwards I saw the woman walking about the ward and learnt that never a drop of matter had formed. I am sure that after a few trials the operator learns to find his way about the axilla quite readily, (particularly if the order of attack which I have just alluded to be followed,) and can lay bare two or three inches of the axillary vessels and scrutinize them with perfect equanimity. Concerning the removal of inguinal glands I cannot say anything of my own experience, as I have only removed one or two in a case of cancer of the penis where I operated about six months ago. The patient, I understand, remains well.

And now, Gentlemen, as a small contribution to the views just advocated as to the local nature of some cancers and the advisability of their free removal, whether they be primary or secondary, I shall shew you two cases. The first patient is a ruddy little man, about fifty five years old, and apparently in the best of health. In June, 1874, over three years ago, he came to the Royal Infirmary with a large glandular mass under the left angle of the jaw. A few months previously a small epithelioma had been removed from his lip at another hospital by a very excellent surgeon, and very well removed

too, inasmuch as it has never appeared again. But soon afterwards the glands began to enlarge. The man took no heed of them, but let them grow till at last they became very painful, and then he poulticed them and speedily the skin broke. He was then forced to seek assistance and when he came to the Infirmary I found a hard lump, as big as a moderate sized orange extending from the left angle of the jaw down the neck. The skin over it was red, brawny, and adherent. In the centre was a ragged hole with gruelly, fœtid stuff oozing out of it, the result of a cancerous gland having inflamed and burst externally. The mass felt very fixed and the skin was involved over an area quite as big as the palm of the hand; yet I feel I could move the lump over the great vessels of the neck, and this gave some encouragement, otherwise I confess I surveyed the prospect of operation rather ruefully. I made a circular incision round the growth through skin apparently quite healthy, leaving even a margin of sound tissue round it. The upper part of this cut was over the angle of the jaw, the lower was quite half way down the side of the neck;—it reached to the posterior border of the sterno-mastoid behind, and close up to the middle line of the neck in front. As the dissection went on it was found necessary to remove most part of the mylo-hyoid muscle, and dig out the whole of the submaxillary gland which had become affected by contiguity. The facial artery and the lingual were cut, a piece of the sterno-mastoid was removed and the external jugular vein was divided, and finally the carotid sheath was uncovered for about two inches and infected glands picked off it. The bleeding was very profuse and the whole process was a long and tedious one. No skin could be drawn together and so the man had to be sent to bed with this enormous hole covered over simply with some lint dipped in carbolized oil. Nevertheless the wound healed with great rapidity and was quite covered in after a few weeks. The patient was very weak for a long time owing to the loss of blood, but this he gradually got over, and by degrees became equal to his work as a day labourer, which he now pursues and which he says he can do as well as ever he did. All that is now visible is a broad white cicatrix, quite

soft and somewhat wrinkled over the side of the neck and even that is partly concealed by his whisker. There has not been the least sign of recurrence since the day of the operation. As a few months of excruciating torment would infallibly have carried him off had the disease been allowed to go on, and as I have over and over again seen men literally condemned to death, who had not a third of the mischief that he had, I am (I hope pardonably) somewhat proud of Thomas Morris.

The second patient I shew you is a tall, robust woman, not far off fifty years of age, and a widow. She had a carcinoma of a somewhat soft and juicy character in her left breast and a little way up from it on the side of the chest, just below the axilla, was an enlarged gland about the size of a walnut. Being tolerably fat and well-nourished the breast was very big, so that after a free removal thereof along with the gland a large wound resulted. I did not chance to use the antiseptic treatment on that occasion, and it happened that no primary union took place. The wound all broke down, extensive suppuration ensued, the matter bagged down by the side of the chest requiring free openings to be made and it was some weeks till all was healed up. I have sometimes, by the way, wondered whether a copious suppuration may not dissolve and wash away all the mass of fat around the breast in which we so often leave cancer roots, and so after all may not prove a blessing in place of an evil. This was done two years ago and the patient remained well for twelve months. Then she came back with a collection of enlarged glands at the top of the axilla. She went into hospital, and I cleared out the whole space very thoroughly. Since then she has been free from any return and has a capital soft cicatrix. She is in excellent health and works hard as a stewardess on one of the Isle of Man boats. Two years have therefore elapsed since the first operation and one since the second, and as there are no more glands in the axilla to become diseased while those above the clavicle seem quite sound, I think we may safely hope for immunity from a local return at all events. I have little doubt that the high axillary glands were really tainted at the time of the first operation, but as

they were not distinctly enlarged to the touch and as the wound I had already made was a very big one, I hesitated to go up into the cavity. I have a very strong notion that it would not be a bad plan when removing a breast *always to clear out the axillary glands whether we can feel anything wrong with them or not*: and I mean to try it. At present we only remove the glands when we can distinctly feel them enlarged and that frequently through a good deal of fat and a thick skin. How do we know they are not already affected? And then the patient disappears and doesn't return till the axilla is full and the great vessels are so mixed up with the disease that we despair about being able to clear everything out and so leave her alone. To make a clearing out of the axilla a necessary part of amputation of the breast would certainly make the latter operation a much more fatal one than it is at present. Indeed, in the way in which it is generally done now, it is an operation which is but very rarely fatal and then only from accidents, such as pyæmia, to which the smallest surgical procedures are liable. But after all I suspect that most men, who have witnessed lingering death from cancer, would, if the case affected themselves, prefer to die rather from the effects of an operation than from the effects of the disease, unless there were some imperative necessity why life even in agony should be prolonged. This may appear a somewhat heterodox idea on paper, but I believe most people in their hearts entertain it.

The first case in which I removed both breast and glands at the same time was that of an elderly maiden lady residing in the country. The operation was performed about five years ago. The patient was fortunately thin, the breast was small and the glands were agglomerated into one round firm mass which lay free in the axilla. I wrote the other day to the medical man under whose care the patient was, and who assisted me at the operation, and asked him what had become of the lady. He replied, "The operation in Miss B's case has been perfectly successful. She is in excellent health and there has not been the smallest indication of a return of the disease. I have regarded the case for some time past as a demonstration of what may be done by operation performed by a skilful anatomist, and the recurrence of the disease, I think, in

"many cases must be due to incomplete extirpation of the affected tissues" I should have hesitated to make public such a flattering letter, but for the fact that the ideas expressed in it are so indetical with those which I am trying to advocate. I think you will give me credit for being sufficiently modest not to take the compliment as a purely personal one, but as applicable to any of my surgical brethren.\*

A much better instance of free removal than any of the cases just described has, however, just been shewn you by Mr. Rushton Parker. The patient whom he introduced had an epithelioma on the left side of the tongue which had spread laterally and backwards also. Mr. Parker laid the cheek open so as to get free access to the oral cavity. He removed one half of the tongue and a piece of the lower jaw, clipped away the hinder part of the upper jaw, carried off part of the tonsil and pharynx close by and took out the sublingual gland which felt suspiciously hard and was just subjacent to the epitheliomatous mucous membrane on the floor of the mouth. Some sloughing of the cheek unfortunately occurred which has left the man with a fistulous opening which, however, he plugs up and is quite indifferent about. But he is perfectly free from the disease, is in good health and earns his living at the Docks as a labourer. Fifteen months have elapsed since the operation ;—time enough for most epitheliomatous growths to recur.

In conclusion, Gentlemen, if you ask why I have taken up your time by expressing opinions which are well known and thoroughly recognized, I can only reply by saying that, while all men profess the doctrine of extensive and sweeping removal, my experience forces me to believe that comparatively few carry it into actual practice. A good tale is none the worse of being twice told ; and my object is not to propound any new thing but to encourage you to persevere in a good work although frequently cast down by disappointing failures. Until we get a specific which will cure cancers, let us

\* Since writing the above, an old lady of 62 called upon me and shewed me a perfectly sound and healthy scar, where I had removed the breast and cleaned out the axilla about fourteen months ago. No trace of return.

push to the utmost the only plan which has hitherto proved at all of avail, and let us disabuse our minds of the notion that the minute we have detected in our patient signs of secondary infection of glands we are straightway to hand him over to a lingering and miserable death without making another struggle to save him. True removal rightly so called will in many instances be found a radical cure, but it must be *true removal*. In performing an amputation, when you have measured out what you think ample flaps it is not a bad plan to cut them even a bit longer:— in removing a cancer, when you think you have fully defined its limits cut far and wide even of them.

## FOUR CASES OF ANTISEPTIC OSTEOTOMY.

By W. M. CAMPBELL, M.D., M.R.C.S.,

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CASE 1.—C. M., an old soldier, was employed at the docks, was brought into the Northern Hospital, suffering from a fracture of the femur at the junction of its upper and middle thirds, with extensive lacerations of the thigh and leg. Owing to these lacerations no extension could be used, and the upper sharp fragment piercing the muscles in the outside of the thigh, added much to the difficulties of the case. A double inclined splint was the only apparatus which could be used, and union was long delayed. When the lacerations were healed, it was purposed to put the limb up in gum and chalk, but no pressure could be borne upon the projecting upper portion of femur which was subcutaneous. I therefore cut down upon it, under full antiseptic precautions, removing about an inch and a half of the sharp oblique fragment. The incision healed without trouble the limb was greatly improved in appearance, and the man was up in a stiff apparatus in about a fortnight.

CASE 2.—Was similar in many respects,—the protruding bone was the tibia a sharp portion of the crest after a compound fracture treated antiseptically. In the same way, and with the same antiseptic precautions, I sawed off the projecting bone, and the result was equally good and speedy.

CASE 3.—Mary C., who had received a Potts fracture about a month before her admission to Hospital. Thinking she had merely sprained her ankle, she took no advice, but staid in bed for some time, and when all pain had gone began to walk about, the result being that she produced eversion of the foot, for

which she at last came to Hospital. When examined on February 3rd, 1877, she rested the greater part of her weight upon the inner margin of the sole of the foot, the fractured fibula was displaced outwards and backwards, the union being firm. An attempt was made to refracture the fibula, but the leverage being so small, this was found impossible. Therefore on February 8th, under the carbolic spray I made a T shaped incision over the fracture, and with a Hey's saw cut nearly through the fibula, even then the callus could not be broken, nor the bone brought into line—so that it was necessary to saw completely through the bone. With an internal splint and a pad over the internal malleolus, I was then able to get the foot into good position. When in position the sawn ends of the fibula were about half an inch apart. The wound was drained by a strand or two of carbolic catgut, sutured, and dressed antiseptically. There was no further trouble with the case, she was up with a gum and chalk bandage in a month, and was discharged on March 16th. She came again into Hospital a fortnight afterwards, as there was a little yielding of the callus, and after another month in gum and chalk, she left with the callus firm, and the foot in very good position. She is now able to walk fairly upon the sole of the foot.

CASE 4.—J. C., a negro sailor, aged 32, was admitted into Hospital and operated on, upon the same date as the last case. His thigh was fractured at sea about a month before his vessel arrived in port. The fracture in the lower third, had united with about three inches shortening, the lower fragment backwards and outwards. The whole thigh was dictated into a bow with the convexity outward, and the callus was of immense size. The limb was utterly useless, and he came into Hospital to have it rebroken. No force which we could employ however was sufficient to refracture the limb. At his earnest desire and after the danger of the proceeding had been explained to him, I proceeded to cut down upon the fracture and chisel the callus. The incision was made upon the outer aspects of the thigh in a line with the obliquity of the fracture. As the line of union did not at once come into view, I was compelled to strip up the periosteum (a proceeding which Volkman condemns.) I then used the chisel to the whole line of union, taking care not to pierce the callus upon the far side, the femur was then easily fractured over my knee, and the limb was put up in a long splint with extending weights. There was considerable hæmorrhage during the operation, which seemed to come from the deep parts of the wound. This having ceased the wound was dressed antiseptically. During the night Mr. Farnell, the House Surgeon, had to change the dressing; the blood having soaked through. The same thing occurred next night, and on the third day the wound was no longer antiseptic. His temperature rose and he complained of pain in the opposite knee, and shoulder. Quinine in large doses (viii grains) was given and his condition slightly improved, the discharge from the wound however having

extremely fetid. On the fifth day there was undoubted effusion into both knee joints, and his general symptoms were those of pyæmia. On the 9th night profuse hæmorrhage took place from the wound, and the House Surgeon only arrived at his bedside to see him expire.

*Post-Mortem Examination.*—Both knee joints were full of pus, and there was a small abscess just over the symphysis pubis. There were no other collections of matter, and the shoulder joint was unaffected. The hæmorrhage was not from the main vessel, nor from any large branch, which were all carefully examined—but seemed to have been from the highly muscular callus. The Medulla of the upper portion of the Femur was in a state of acute inflammation.

*Remarks.*—Without some such precaution as we have in Prof. Listers' system carefully carried out, I am strongly of opinion that the foregoing operations would have been unjustifiable. The fatal termination of the last case shows the danger attending hæmorrhage in antiseptic surgery—the two attacks of intermediary bleeding were sufficient to cause the failure of all antiseptic precautions, and undoubtedly were the causes of the pyæmia and death.

# ABSTRACT OF THE PROCEEDINGS OF THE LIVERPOOL MEDICAL SOCIETY.

*DURING THE SESSION 1875—76.*

**By CHAUNCEY PUZEY.**

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*October 7th.*—Mr. McCHEANE, the President of the Society, delivered the introductory address, in which he commented upon the leading topics of interest which had come under the notice of the profession during the recess. He commenced by a reference to the meeting of the British Medical Association, at Edinburgh, at which among other matters, the question of the admission of lady members to the Association had been taken into consideration; and at which probably the most interesting subject of discussion had been that of Antiseptic Surgery, which was very completely and practically demonstrated by Professor Lister. He alluded to the invitation sent to the Association by the profession of Liverpool to hold their next meeting in this town, and the reasons for its not having been accepted. He then made some observations upon the recent controversy concerning the use of certain anæsthetics, and the sensational correspondence which had appeared on the subject in the leading London papers, and intimated his belief that the question would be fully discussed by this Society during the coming session. After drawing attention to the disparaging statements which were too frequently made concerning the condition, social, moral, and sanitary of the town of Liverpool, and pointing out how, to a great extent, these were unjustified by facts, he passed on to the consideration of the

prevailing tendencies of the present day with regard to the general treatment of disease, and concluded by calling upon the members of the Society to do their part in advancing the usefulness and honourable position of medical science.

*October 21st.*—MR. PUZEY, showed a solid Bursa Patellæ 14 inches in circumference, which he had successfully removed from a woman, aged 34. The operation, which was rendered somewhat tedious by the universal and firm adhesions of the tumour, was rendered bloodless by means of Esmarch's bandage. The growth had existed for 2 years.

Dr. W. CARTER, exhibited (for *Dr. Alexander*) two urinary calculi. One, of Oxalate of Lime, weighing over an ounce occurred in a man, aged 29, and was discovered whilst the patient was under treatment for stricture. It was successfully removed by the median operation. The other, composed of uric acid, covered with phosphates, was removed, *post-mortem*, from between the mucous and muscular coats of the bladder. The patient, a female, was admitted into hospital in a sinking condition.

Dr. MORRISH exhibited an enormous cancerous mass from the abdomen of a child, aged six years.

Dr. WATERS read the particulars of a case of acute Rheumatism with high temperature, treated by the cold bath. A girl, æt. 17, had been four days under treatment (in the Royal Infirmary) when, the temperature having gradually risen to 104.7, the respirations being 40, she was immersed in a bath of 95° F. She was left in the bath until the temperature of the water was 95 degrees. When taken out her temperature was 99 degrees F. The patient complained of cold after it, but there was no shivering. The temperature rose again and the bath was repeated four times within 48 hours, each time with satisfactory result; and the patient made a good recovery, after four weeks treatment in the Infirmary.

Mr. SHADFORD WALKER read a paper "On an improved operation for the treatment of conical cornea and Staphyloma."

*November 4th.*—Dr. SPRATLEY showed a horse shoe kidney.

Dr. WIGLESWORTH showed two gall-stones, each about the size of a nutmeg, articulating with each other, obtained from a patient, æt. 67, who had died from cancer of the liver. There had been no symptoms of gall-stones.

Mr. PUGHE showed double conjoined placentæ; the one healthy, the other decomposed; one child being born alive.

Dr. DAVIDSON exhibited specimens of thrombosis of the femoral vein with pulmonary and cardiac embolism. The case from which they were taken was one of "white-leg," in which the symptoms commenced 3 weeks after confinement, and the patient died with the usual signs of pulmonary infarction. The uterine sinuses were plugged, and the thrombi extended directly into the left iliac veins.

Dr. LYSTER exhibited the larynx of a child 18 months old, the seat of syphilitic membranous deposit. Tracheotomy had been performed; but the patient died 24 hours after. The child's father had syphilitic sores about his mouth, and had been in the habit of giving his infant bread chewed in his own mouth.

Dr. SPRATLEY gave particulars of two cases of Trismus successfully treated with Gelseminum. In the 1st case, that of a woman, aged 37, a patient in the Birkenhead Borough Hospital, the symptoms came on 14 days after a wound of the forehead. The symptoms were relieved at first by Chloral Hydrate, but afterwards returned. Chloral was again given, this time without success; when Tincture of Gelseminum in doses of 20 grains was prescribed to be taken three times a day. The mouth at this time could not be opened. After four days of this treatment, some nausea and drowsiness supervened; and at the same time the rigid muscles began to relax; and a week later, the patient could feed herself, and eventually made a satisfactory recovery.

The 2nd case was that of a man, æt. 24, poorly nourished, admitted into hospital for laceration of arm and crushed hand,

necessitating amputation of two fingers. On the fourth day secondary hæmorrhage occurred, and on the nineteenth the arm became rigid. On attempting to straighten the fingers, the whole body was seized by tonic spasm. A day or two later, the jaws became locked. Tincture of Gelseminum was given in 20 grain doses, and shortly increased to 30 grains. The trismus however, became worse, and food was given with the greatest difficulty; the muscles of the thigh and arm became rigid, and there was slight pleurosthotonos; pulse 130. The dose was then increased to 45 grains. (This was on the 10th day after symptoms of Trismus appeared.) Next day there was slight drowsiness, and two days later the jaws could be slightly separated. From this time there was gradual improvement, and a fortnight later, the man was able to leave the hospital; having taken Gelseminum for four weeks, the time during which the symptoms lasted.

Dr. OXLEY and Mr. HIGGINSON exhibited Ether inhalers; the latter showed one of the old forms; the former showed one used at the present time in America.

Dr. CHALMERS related the particulars of a case in which he had removed from the bladder of a female child a calculus the size of a large nut, by dilatation of the urethra.

Mr. RUSHTON PARKER brought before the Society a boy upon whom he had operated for compound fracture of the lower jaw in the method described by Mr. H. O. Thomas, of this town. He related the particulars of the case, described the operation, and showed the instruments used. The case was complicated by a fracture of the upper jaw which was not interfered with; union of the lower jaw took place in ten days, but the wire was left in for a month.

*November 18th.*—Dr. GAMES showed the parts from a case umbilical hernia in an infant; part of the small intestine had escaped from the umbilicus; as that contracted, the gut became strangulated, and then gave way; there was then a considerable

discharge from the bowel, but the child died, when three weeks old.

Dr. CAMERON exhibited a specimen of the perforation of the Vermiform Appendix with malformation of the Cæcum, which had been taken from the body of a young man who died with symptoms of intestinal obstruction. At the p. m. examination there was found general peritonitis, perforation of the Appendix, which was very long; and a small angular cul-de-sac in place of the Cæcum which was absent. There were no foreign bodies found.

Dr. BRAIDWOOD exhibited a specimen of intus-susception of the whole of the colon. It occurred in a child, æt. 3, perfectly healthy until the day of its death. Diarrhæa came on suddenly after its dinner, the stools were mixed with blood, tenderness of the left half of the abdomen was noticed, rapid collapse ensued, and the child died the same evening. The whole of the large intestine seemed to have disappeared, the ascending and transverse colon being completely intussuscepted.

Dr. WIGLEWORTH related the particulars of a case of puerperal convulsions in which, after other methods of treatment had failed, recourse was had to the administration of Chloroform. Anæsthesia immediately relieved the symptoms, and after it had been kept up for nearly six hours, the fits ceased, consciousness gradually returned, and the patient made a good recovery. At the height of the convulsions, the urine, which had a few hours before been found healthy, was tested, and proved to become almost solid on boiling.

Dr. GLYNN brought before the meeting a patient affected with progressive muscular atrophy. He was a ship steward, æt. 30. The muscles of the balls of both thumbs were atrophied, and there was in these parts complete insensibility to the faradaic current. Sensibility to heat and cold was impaired in these parts. The fingers were partly flexed; no other muscles were affected. The man had a large spleen, had suffered from ague two years before, and said that after a sharp attack of ague he one day suddenly lost his sight and remained blind for several days, when the sight

suddenly returned. About this time he noticed that his hands were weak, and that the balls of his thumbs were flatter than usual. Dr. GLYNN thought it probable that the case was one of a symptomatic affection of the motor cells of the anterior cornua. So far, as M. Charcot had pointed out, the prognosis was more favourable, than if it was a case of primary atrophy of these.

Dr. CARTER related the particulars of a fatal case of rheumatic fever with hyperpyrexia in which immersion in the cool bath had been tried, but without any improvement.

Mr. RUSHTON PARKER then described the p. m. appearances in four cases of fatal rheumatism with hyperpyrexia. The highest temperatures in these cases had been respectively, 106, 107, 109, 109. In all there was a pulpy state of the liver, soft spleen, and fatty condition of the kidneys. He pointed out that the condition of the liver was one that might be easily overlooked, but was readily recognised if looked for. The same condition of these organs was observable in cases of Phlebitis. Septicæmia and many forms of surgical injury in which high temperature had been noticed during life.

*December 3rd.*—Dr. LEWIS brought before the Society a boy whose body was covered with spots resembling those of puerpura, but who was in perfectly good health.

Dr. LUPTON exhibited Dr. Matthews Duncan's Cephalotribe, and explained its action; he also showed an infant whose head had been crushed by the instrument.

Dr. IRVINE showed a specimen of syphilitic disease of the lung. The diseased portion was the seat of fibroid degeneration, and was firmly adherent to the pleura. The patient, a male, had contracted syphilis in early life, had suffered from the usual secondary and tertiary affections, and died with symptoms of commencing paraplegia. The spinal cord was found hardened in some parts, softened in others. There were fibroid deposits in the liver.

Dr. ALEXANDER showed a chain of lymphatic glands of the neck affected with primary malignant disease. There was a history of six months duration of the the disease ; the patient died in the Workhouse Hospital. The pectoral muscles were infiltrated.

Dr. ALEXANDER also showed a specimen of double aneurism of the arch of the Aorta, interesting as having been treated at the Northern Hospital, under Dr. Waters' care, 5 years before. In that hospital he had been treated with Iodide of Potassium and was discharged as cured, *i.e.*, he was in good condition, there was no pulsation, dyspnœa, or dysphagia. From that time he had been free from all symptoms until quite recently, when he was admitted into the Workhouse Hospital, and soon died from exhaustion, but without symptoms of pressure. At p. m. examination the Aorta was found excessively diseased and dilated ; there was one aneurism close to the Aortic Valves and another beyond the roots of the large vessels spinging from the arch.

Mr. R. A. H. WOOD showed a Hodge's pessary which had been left for 7 years in the vagina without removal.

Mr. WOOD also exhibited an enormous pedunculated tumour which had been attached to the uterus ; he also described his unsuccessful attempt at the removal thereof. Abdominal section was performed at the urgent request of the patient, but universal adhesions and excessive hæmorrhage from dilated viens was the cause of failure ; and the patient died shortly after.

Dr. GLYNN related the particulars of a case of pericardial effusion, in which paracentesis was practised three times. The patient was a boy, the history of the case was indefinite ; but the signs of pericardial and also of right pleuritic effusion well marked. Tapping was performed the day after admission, when the boy was in a very prostrate condition, disposed to be delirious, with pulse 152, respirations 28, temperature 102-4. The aspirator needle was introduced in the 4th intercostal space close to the sternum, and 12 ounces of serum drawn off. Temporary im-

provement took place, but two days after, a relapse occurred. The aspirator was again used and 6 ounces of reddish serum were drawn off. Very little relief occurred, and two days later, 6 ounces more of serum were drawn off, but the boy sank on the fifth day after the first operation.

Mr. EDGAR BROWNE read a 'Note on the most recent views respecting Hemipopia;' in the course of which he referred to the remarks of Dr. Hughlings Jackson on the association of hemipopia with hemiplegia, and discussed the different views held with respect to the decussation of the optic nerve-fibres; illustrating the subject by means of a diagram.

Dr. WIGLESWORTH read a paper on "Puerperal Eclampsia." He commenced by observing that the Uterus under its increased development and at a time prone to phenomena could be a source of so much irritation as might culminate in convulsions. He then referred to the influence of the kidneys and of renal disease in producing such convulsions; pointing out that Albuminuria and Renal Disease are not strictly convertible terms, inasmuch as (1) Albuminuria may be present without the existence of kidney disease. (2) Puerperal convulsions may end fatally, and yet no disease of the kidneys be found. (3) Considerable albuminuria may be found, and yet the patient may escape without convulsions. He then described the views Frerichs, Braun, and Leishman as to the relation between puerperal convulsions and kidney disease. He observed that convulsions are not as a rule an accompaniment of renal disease; and that on the other hand, the premonitory signs of Bright's disease are nearly absent in cases of puerperal eclampsia. It was quite in accordance with pathological laws to infer that some of the cases of albuminuria associated with pregnancy were due to deranged functional activity, which might be caused, for instance, by pressure, as Basham and others have shown. He referred to observations of Basham, showing that there may be albuminuria without organic disease of kidney, and that in such cases it seemed probable that the depurative action of the

kidney upon the blood would be impaired. He then pointed out that although puerperal eclampsia sometimes arose from renal disorder, in many cases the cerebro-spinal system itself was at fault. The causes of convulsions generally might be arranged in two classes:—those which arise from changes within the nervous system, and those which are due to agents acting from without. In considering the part played by the nervous system in puerperal eclampsia, Dr. Wigglesworth would arrange them in three divisions, (1) Deranged action of the centres themselves. (2) Reflex action caused by irritation propagated to the nerve-centres. (3) Nervous tension liable to be deranged by systemic or emotional causes. With regard to the first, Rosenstein holds that under the influence, of parturition, through pressure on the aortic circulation, œdema is produced, and consequent cerebral anæmia. Such a theory could not explain those cases which occur after labour, when all muscular efforts have ceased. For reflex irritation to produce convulsions, a peculiar susceptibility of the nervous system to receive impressions must exist; and it was under such a combination that Dr. W. considered that the majority of cases of puerperal eclampsia were to be included. He then dilated upon the high nervous tension which may be presumed to have existed in many cases of puerperal convulsions; directed attention to Schröder van der Kolk's observations on the part played by the medulla oblongata in the production of reflex movements, and pointed out how those observations helped to elucidate many of the phenomena connected with puerperal eclampsia. Finally he remarked that if our treatment is to be successful, it must be dependent on the correctness of our diagnosis, and that the view enunciated of the part played by the cerebro-spinal system is strengthened when the effect of Chloroform is considered, for by it the mortality in puerperal convulsions has been diminished to the extent of one half. In uræmic convulsions Dr. W. considered that bleeding was the sheet-anchor.

*December 16th.*—Dr. DICKINSON exhibited the parts affected in a case of intussusception. The woman from whom they were removed had been ill 5 weeks; the symptoms which had at no

time been urgent had been constipation and tympanitis accompanied by very little pain; and for 8 or 9 hours before death, stercoraceous vomiting. There was invagination in the neighbourhood of the cæcum, but obstruction was not complete.

Mr. PUZEY showed a large adenocele of the breast, with a number of cysts scattered through it. The mass, about the size of two fists, was removed by enucleation, from a woman about 38 years of age, the mammary gland being left.

Dr. OXLEY exhibited a heart from a case of pericarditis ensuing upon periostitis in a child 7 years of age.

Dr. F. IMLACH showed a heart with incomplete septum ventriculorum; the foramen ovale was open; ductus arterious still open. There had been no cyanosis during the 6 weeks of life.

Mr. RUSHTON PARKER exhibited a bladder ruptured apparently from spasmodic retention; it had given way 48 hours before death. The circumstances attending the rupture were not known. He also showed a specimen of stricture of the rectum, probably of an inflammatory nature.

Dr. RAWDON introduced a girl, æt. 14, who had suffered from hypertrophy of the skin of the neck. This hypertrophy had commenced, when the child was 3 years old, in a mole which existed at birth at the lower part of the cheek. The skin, which was of darker colour than the surrounding integuments, had been recently the seat of darting pains, and the arm of the corresponding side had become rather weak. The redundant skin which hung in loose folds was removed by excision; the dissection was carried deep into the subcutaneous tissues and many curious cords (altered vessels?) were removed, about the size of crow-quills; the hæmorrhage was slight. The flap removed was quadrilateral, about 8 inches in length; the wound was closed by button sutures; the edges not being quite brought together. The result of the operation was most satisfactory.

Mr. FRED. W. LOWNDES read a paper on the subject of "The Contagious Diseases Acts, their history and some of their results; and ought they to be applied to Liverpool." Having during his last occasion occupied himself in visiting the military and naval station of Chatham, Windsor, Plymouth and Devenport, Mr. Lowndes was in a position to speak from a practical observation of the working of these acts, and at the end of a long and interesting paper on the subject he gave a very decided opinion that it was in every way desirable that the acts should be more generally applied; and especially in a large sea-port like Liverpool.

*January 20th, 1876.*—Dr. TURNBULL showed an aneurism about the size of a goose's egg, arising from the aorta at the site of origin of the right renal artery, which appeared to come off from the aneurismal sac itself. This artery was diminished in size and the kidney was atrophied. The left renal artery and kidney were enlarged. The aneurism was filled with fibrinous clot; and the patient died of pneumonia wholly unconnected with the aneurism.

Mr. R. A. H. WOOD exhibited an unusual enlargement of Prostate Gland removed from the body of a man who had suffered from repeated attacks of retention of urine and hæmaturia, and died from exhaustion. On p. m. examination the bladder was found full of coagulated blood; the base of the bladder was encroached upon by the prostate which was enlarged in *four* lobes, each about the size of a large walnut; a layer of vascular membrane resembling the Pia Mater was spread over the tumour, and the vesical walls were considerably hypertrophied.

Dr. DAVIDSON exhibited the heart of a patient in whom a presystolic murmur had been heard occasionally; vegetations were present on the valves, and principally on their auricular surface. He also exhibited a heart in which tricuspid regurgitation had been detected during life. The valve admitted four fingers.

Dr. ALEXANDER showed two large masses, or chains of malignant enlargement of lymphatic glands, from the side of the

neck. In one case, which had apparently existed only six months, death arose from pressure. In the other case, that of a man, *æt.* 36, and only admitted into hospital three days before death, the axillary glands had been first affected; and from this situation, a chain of enlarged glands extended up the side of the neck to the base of the skull. The man died of pleurisy.

Dr. ALEXANDER also showed a specimen of displaced kidney. The abnormality was threefold; viz., as regards position, shape, and blood-supply. It rested on the promontory of the Sacrum; it was crescentic, almost circular in shape, with a bifid ureter; its blood supply was derived from the Aorta by one branch, and from the left common Iliac, by two; each branch being accompanied by a vein. The other kidney was shrunken. The supra-renal capsules were normal.

Dr. DICKINSON showed the brain of a female patient of his who had died comatose with complete *left* hemiplegia. The case was peculiar inasmuch as a p. m. showed a large clot spread over the *left* hemisphere, under the arachnoid; there being no lesion on the right side of the brain.

Dr. E. WHITTLE showed a very vascular small scirrhus of the mammary gland of three months growth.

Dr. LYSTER showed a brain with a large recent clot in the right lateral ventricle. It was removed from a man, *æt.* 72, who had been suddenly seized with unconsciousness and left hemiplegia, and died comatose two days after the attack. The Arachnoid space and left lateral ventricle also contained a large quantity of bloody serum.

Mr. GEORGE WALKER brought before the Society a woman, *æt.* 25, who had been under his treatment for chronic abscess of the orbit, the result of a blow on the eye, received 7 years before. When Mr. W. first saw her, the eye was much protruded and vision of that eye was so much affected that she could hardly count fingers. He detected fluctuation just below the upper edge of

the orbit, and let out two drachms of thick pus. No bare bone was felt. This was done three weeks ago, and when brought before the meeting the eye had considerably receded and vision was improving. There was still some discharge from the wound.

Dr. LYSTER brought forward a woman æt. 56, affected with Morbus Addisonii. The case was well marked, and showed especially well the dark mottling of the buccal mucous membrane described by Trousseau as seen in the mouth of a bull-dog. The patient dated the commencement of her illness to a fright sustained 2 years before, after which vomiting, diarrhæa, and great exhaustion ensued.

Dr. WALLACE read a paper "On the management of the 3rd stage of Labour;" in which he considered the views of past and present gynæcological authorities on the subject of the position of the Placenta *in utero*, and of the mode of its expulsion, and showed that whatever may have been the site of its implantation, the tendency in a natural labour is for the edge of the Placenta to present at the os uteri; and that any other presentation must be due to injudicious traction on the cord. He then discussed the subject of interference or non-interference in the removal of the Placenta; glancing at the directions given on this points in the published works of writers on obstetrics; and for his own part advocating patience rather than active interference, except in cases of excessive hæmorrhage. He concluded with some observations on the treatment adopted after the removal of the after birth, again impressing the advisability of trusting to nature, and speaking in condemnation of the administration of drugs or stimulants in ordinary cases, as also of the use of the binder or compress, which he considered was more likely, as generally applied, to do harm than good.

*February 3rd.*—Dr. GLYNN showed a specimen of Chronic Inflammatory Induration of the stomach, resembling epithelioma. It occurred in a woman, æt. 30, who was admitted into the Infirmary

in a pulseless state, and died next day. There was a history of intense pain in the epigastric region for six months previously.

Dr. WALLACE showed a specimen of Aneurism of the Arch of the Aorta taken from a man, *æt.* 34, who, two years ago, whilst rowing, had been suddenly seized with such violent pain in the chest that he fainted and was for some time unconscious. After this, there were no further symptoms, except slight attacks of dyspnœa, until six months before death, when a pulsating tumour was noticed near the Manubrium Sterni. This tumour was about pointing externally, when the man died from internal hæmorrhage.

Dr. W. CARTER showed a heart into which an Aneurism of the Aorta had burst. The Aneurism, which was about the size of a hen's egg had forced its way in between the right auricle and ventricle, and had burst into the latter just below the auriculo-ventricular valve.

Dr. DICKINSON exhibited a heart with extensive vegetations on the aortic valves.

Dr. CORMACK related a case of secretion from the mammary glands, in a female infant, 14 days old. In this case there was a sanguineous discharge from the genitals.

Dr. CAMPBELL related the particulars of a case in which serious and unusual symptoms followed the introduction of solid Nitrate of Silver into the uterus. The case was one of subinvolution of the uterus in a multipara who had suffered for some years from retroflexion. Ten grains of the solid caustic was passed into the uterine cavity and left there. Immediately, violent abdominal pain and collapse ensued. Stimulants and afterwards opiates were administered, but for several hours the symptoms were most alarming. For several days nausea and occasional vomiting continued, and for three weeks the patient suffered from intense abdominal pain, and subsequently made a very tedious recovery.

Dr. DICKINSON read a short paper on "The appearance of Paralysis on the same side as the lesion of the Brain." In it he gave the clinical and pathological features of a case which had been lately under his care (The specimen was shown at a previous meeting.) A woman, æt. 23, was admitted into the Northern Hospital, having been found lying unconscious in the street. Slight movements were made with the right hand and arm, but the left side appeared perfectly devoid of power or sensation. The left pupil was widely dilated, the right, if anything, contracted, but there was no deviation of the features. The next day the hemiplegic symptoms were less; the left side weak, but the patient able to move and to articulate; the pupils were equal and normal; but there was ptosis of left eyelid and external strabismus of that eye. During the week following, there was slight improvement, but on the 8th day sudden stupor supervened, and the patient became comatose, and died on the 9th day after the first seizure. During this last period there was slight sensibility and reflex movement of the right, none of the left limbs.

A p. m. examination disclosed a large blood-clot (about 3½ ounces) on the surface of the left hemisphere, lying between the Dura Mater and the Arachnoid. The hæmorrhage had proceeded from a branch of the Middle Cerebral artery, in the intra-parietal fissure. The brain substance was undisturbed, and the effusion confined entirely to the left side. A clot the size of a horse-bean was found lying at the bottom of the Sylvian fissure, pressing on the trunk of the left 3rd nerve. The whole of the brain-substance, and its bloodvessels, the Cerebellum, Pons, and Medulla, were carefully examined and with the exception of 3 drs. of Serum in the right Ventricle, no other lesion was found. The decussation of the anterior pyramids in the Medulla appeared normal.

Dr. DICKINSON then reviewed the principal features of the case and more especially the peculiarity of the paralysis occurring on the same side as the lesion of the brain; as to which point he thought the evidence was clear from the post-mortem appearances, which failed to show any lesion on the right side which could account for

the symptoms. In considering this point he made reference to and read extracts from an article recently written on this particular point by Dr. Brown-Séquard, published in the "Lancet," of January 15th and 29th, 1876.

Dr. W. CARTER read notes of two cases of Ascites successfully treated under his care in the Royal Southern Hospital. In both these cases Copaiba Balsam was used. In the first case, the capsules were given three times a day, with apparent benefit, for six weeks, when these were discontinued in consequence of nausea being caused. Various other remedies were used in this case, and paracentesis abdominis was performed on three occasions; and the patient, a seaman, aged 45, ultimately left the Hospital, apparently completely cured. In the other case, that of a soldier, aged 36, 15 grains of the resinous extract of Copaiba was given three times a day, with a marked effect upon the quantity of urine secreted; after a fortnight, the capsules were substituted and these were taken with slight intermissions (when nausea was excessive) for nine or ten weeks, and at the end of three months the man went out cured; at all events as far as the ascites was concerned.

*February 17th*,—Mr. PAUL showed a piece of bone removed from the calvarium of a sailor, æt. 66. There was a somewhat indistinct history of old syphilis; disease of the bone had been going on for 12 years. The piece removed involved the whole thickness of the bone for an extent of 7 inches by  $6\frac{1}{2}$  across the vertex. The man also was shown; he presented a peculiar appearance, almost as if the crown of the head had been sliced off; the Dura Mater was covered with healthy granulations; and of course pulsation was evident all over the surface. The mental faculties were unimpaired.

Dr. CATON exhibited some casts from a case of plastic bronchitis occurring in a boy, aged six years; each cast consisting of three or four tubes of membrane arranged concentrically, one within the other. The patient had had a similar attack a year ago.

Dr. LYSTER showed a calculus, the size of a small nutmeg, which he had removed from the urethra of a woman, aged 50. It had become sacculated in the urethra, so as to encroach upon the Vagina, leaving the urethra free for the passage of urine. It was removed by dilatation of the urethra, and dislodgement by means of a finger in the Vagina.

Dr. DAVIDSON exhibited specimens of Embolism of Arteries of the Brain, Spleen, and Kidney from the body of a man, who died hemiplegic. Two days before death, gangrene commenced in both feet, and at the end of the nose. A faint diastolic murmur had been occasionally audible at the apex of the heart and the heart's action was rapid and irregular. A p. m. examination revealed complete softening of the corpus striatum and of the whole of the anterior lobe of the left side of the brain. The end of the left internal carotid contained a firm semi-organised clot, extending into the anterior and middle cerebral arteries. There was no atheromatous disease. The mitral valves were thickened and adherent; the orifice much contracted. In the left auricle was a loose fibrinous clot. In the spleen were two depressed cicatrices (result of previous embolisms); and at another part, section showed patches of pale mottling with plugging of arteries (recent embolisms). A large branch of the left renal artery was plugged by a considerable clot. The right kidney was completely atrophied. A clot was found in the right anterior tibial artery. The other arteries were not examined. The veins of the lower extremities were much distended with dark coagulated blood.

Mr. REGINALD HARRISON read a very elaborate paper "On the advantages of Ether over Chloroform as an Anæsthetic."

After a few preliminary observations on the importance of duly discussing the relative advantages of anæsthetics, he submitted for the consideration of the meeting four propositions;— 1st, Is Ether a *safer* anæsthetic than Chloroform? 2nd, Is the administration of Ether attended with inconveniences, or followed by consequences, such as to render it an undesirable anæsthetic? 3rd, Are there

cases in which Chloroform is to be preferred to Ether? 4th, Are we acquainted with other modes of producing anæsthesia, which promise greater safety? He considered these four points at length, and gave as his opinion that Ether was so unquestionably safer, that any slight inconvenience in its administration was not to be considered; that Ether properly administered, was suitable in every case in which Chloroform was applicable, and that for prolonged operations, no anæsthetic had yet been discovered which could be said to possess greater advantages than Ether.

*March 2nd.*—Dr. CAMERON exhibited a specimen of Thoracic Aneurism illustrating that form of the disease in which the symptoms were entirely due to pressure on the left recurrent laryngeal nerve. The patient was admitted into Hospital suffering from severe paroxysms of dyspnœa, accompanied by laryngeal stridor, and loud ringing cough. No other symptoms were present. Death occurred suddenly after one of the attacks of dyspnœa.

The aneurism was found arising from the commencement of the transverse portion of the arch of the aorta, and was somewhat larger than a walnut. It had compressed the recurrent laryngeal nerve, which was shown attached to the sac.

Dr. LYSTER showed an Aneurism of the Thoracic Aorta from a Workhouse patient, an old woman of 70, who was under treatment for diarrhœa and vomiting, with much febrile disturbance, and who died suddenly whilst getting out of bed. On p. m. examination, a large quantity of blood was found in the pleural cavity. There was fusiform dilatation of the ascending and part of transverse portion of the arch of the aorta; the walls of the aorta were thin and contained calcareous plates. At the middle third of the thoracic aorta, was a large sacculated true aneurism, which by a process of natural cure had become almost solid. At the lower part of this aneurism was a rupture which had caused death.

Dr. CLARKE also showed a specimen of Thoracic Aneurism.

Dr. ALEXANDER showed some bones from a case of extrauterine

foetation occurring in a woman, æt. 30, who died in the Workhouse Infirmary, after suffering for two months from diarrhœa. The p. m. appearances in connection with the specimen were as follows ; —A band of old-standing lymph crossing, but not adherent to the lower part of the Ileum, approximated, but did not constrict, the intestinal walls. A short distance higher up the Ileum, the skeleton of a foetus was found glued to the out side of the intestine. There were numerous adhesions between the different pelvic viscera.

Dr. ASHBY showed two pelves, deformed through osteo-malakia, removed from subjects in the dissecting-room.

Mr. GEORGE WALKER related the particulars of a case of Fibroid Tumour, occurring in the stump of an enucleated eyeball. The man, whose left eye-ball had been removed five years before, presented himself to Mr. Walker with the orbit filled up by a fungoid growth which appeared to have infiltrated the skin for about  $\frac{1}{2}$  an inch beyond the edge of the orbit. There was thickening (inflammatory) even beyond this, which however subsided after a few days dressing with lead lotion. A "tunnel" having been cut by means of knife and scissors until the apex of the orbit was reached, a long straight blunt-pointed bistoury was introduced into it, and made to cut all round the tumour, close to the orbital wall ; the mass thus removed being pyramidal, or rather conical in form. The periosteum was then stripped from the bone, and the cavity plugged, to stop bleeding. The patient made a good recovery ; and appeared before the meeting with the wound almost healed. Mr. Walker was doubtful as to the exact nature of the growth, but was inclined to class it with rodent ulcer. He recommended this mode of treatment in preference to the use of Vienna paste, or other caustics.

Drs. ALEXANDER and BARR contributed a joint paper on "A peculiar form of Pseudo-paraplegia, ending in complete paralysis, from Caries of the Vertebrae."

From the very full account of this case given in the paper

(which cannot be sufficient condensed for this summary), it would appear that the subject of this report, a sailor, æt. 22, suffered from such anomalous symptoms that for a long time it was a matter of question whether he was really suffering from organic disease or from spurious paralysis; although there was undoubted prominence of the 11th dorsal spinous process. The man had been under treatment on and off, for 3 years, in different hospitals in various parts of the world; in the latter part of 1874, he came under Dr. Barr's observation in the Northern Hospital, and in the midsummer of 1875, he was admitted into the Workhouse Infirmary, where he died, 6 months later, with well-marked spinal caries, and total paraplegia.

A p. m. examination showed that half the bodies of the adjacent portions of the 11th and 12th dorsal vertebræ with the intervertebral disc, had disappeared, the remaining halves of these bones being condensed and hard. The cord at this part was soft and pale, surrounded by thickened membranes and pushed to the posterior part of the canal by inspissated pus above, and below by a large piece of bone. The anterior vertebral ligament was thickened; the costo-vertebral and transverse joints destroyed; the articulating surfaces, as well as the bone adjacent, being rough and carious.

In the progress and termination of the case, many interesting points arose, which were noted by the authors of the paper. The reasons which gave rise to the suspicion that the symptoms were assumed were (1) That there was no *true* paraplegia; (2) The symptoms were such as could be assumed; (3) The patient presented the appearance of a malingerer, being a stout, well built man, (when first under observation) with firm muscles, good appetite, sleeping well, and with healthy complexion; and there being an entire absence of objective symptoms, excepting an apparent difficulty in walking, (4) Because all his sensations were normal; so also electro-contractility and electro-sensibility; (5) Because he had been dismissed from the American navy as a malingerer. Later on, when the man began to lose flesh and to look ill, the question of

hysteria was considered, but dismissed as inconsistent with the local symptoms; and it then became evident that true paraplegia was present. Looking back to the earlier history of the case as viewed by the light of its termination, the authors of the paper now concluded that at first it was a true case of Pseudo-paraplegia, the symptoms of which were solely referable to the antero-lateral columns, and probably due to slight pressure on these parts causing irritation rather than destruction of the motor conductors. As disease extended, the symptoms likewise increased and became more intelligible, and true paralysis set in; and eventually the grey matter became involved, thus causing abolition of reflex action and the many subjective symptoms from which the man latterly suffered. The fact that tactile sensibility remained until nearly the end, showed that the posterior columns were little if at all affected.

*March 16th.*—Dr. BRAIDWOOD showed a number of gall-stones removed, post-mortem, from the gall-bladder of a woman, æt. 46, who was found to be the subject of cancerous infiltration of the omentum. The gall-bladder and common duct had greatly thickened walls, and were filled with greenish pus and about 200 gall-stones. There were adhesions between the abdominal walls and the subjacent viscera; and the liver, stomach, and part of intestines were adherent "*en masse*."

Mr. PAUL exhibited a specimen of cerebral softening, from a woman, æt. 30, in whom, 14 weeks before death, paralysis of the left 3rd nerve occurred, and a fortnight later the right 3rd nerve became similarly affected, and subsequently the patient, after a fit, became aphasic; and was admitted into the Infirmary suffering from these symptoms with partial left hemiplegia.

At the p. m. examination, the roots of both 3rd nerves were found thickened by new tissue, apparently syphilitic. Several cerebral arteries, especially the right middle cerebral, were thickened and filled by thrombi. At the origin of the left middle cerebral artery, was a minute aneurism which had given way, and given rise to considerable hæmorrhage on the surface of the brain.

There was a large patch of softening in right corpus striatum and thalamus opticus; a small patch in left corpus striatum.

He also showed a specimen of fibroid phthisis.

Dr. SHEARER read a short paper "On the amount absolute of blood in the human body." He showed that the estimates have been gradually falling from as much as 40lbs. to 8lbs. for the ordinary adult; which latter figure he would adopt as probably nearer the truth than any of the other estimates; and he considered this view substantiated by estimates derived from the examination of the pig, whose physical conformation approached that of man. He then referred to the estimates of different physiologists, and detailed the method which he had adopted in arriving at his own conclusions on the subject; he thought that further observations would establish the fact that previous estimates are too high, and that 1-20th about represents the proportion of blood in the human body.

*March 30th.*—Dr. CAMERON showed a specimen of Cirrhotic Liver.

He also showed a specimen of Encephaloid Cancer of the Stomach and Colon, causing adhesion and apparently a fistulous communication between these viscera. The case was remarkable inasmuch as the only symptoms during life were constant severe purging and occasional vomiting; and as the patient, a sailor, had frequently suffered from dysentery and had recently suffered shipwreck, he was considered to be suffering from the sequelæ of tropical dysentery.

Mr. GEORGE SNAPE exhibited and explained the mode of working Clover's apparatus for the administration of Nitrous Oxide Gas followed by Ether.

Mr. FAY read a paper "On some results of dental lesions;" with the object of illustrating the connection which exists between general and dental surgery. After referring briefly to the subject of dental caries, which he considered to be mostly of constitutional

